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AMERICAN JOURNAL OF SCIENCE

[FOURTH SERIES.]

Art. XII. — *The 27-Day Auroral Period and the Moon;* by H. Helm Clayton.

Several years ago my attention was called to a tendency of auroral displays to recur at intervals of 27 days. This is illustrated by the following cases taken from the records of the Blue Hill Meteorological Observatory:

Table I.

	Interval.	Other Auroras.		Interval.	Other Auroras.		Interval.	Other Auroras.
1892			1893			1895		
Dec. 23	28		Dec. 29	29		Jan. 19	27	
1893			1894			Feb. 14, 15, 16	27	0
Jan. 20	26		Jan. 27	26	Jan. 2	Mar. 14	27	
Feb. 15	55	Feb. 4	Feb. 22, 23		" 3	Apr. 10		
Apr. 11	26	" 5			" 4			
May 7								

This table gives all the auroras observed between the first and last dates in each of the series. In these instances the recurrence of auroras at intervals of approximately 27 days seems too well marked to arise merely from chance coincidence. For example, in the last series there are four auroral displays at exact intervals of 27 days and no intermediate displays. The probability of a casual arrangement of the auroral displays in this manner is inversely as the cube of 27, or as 1 to 19,683. That is, out of 19,683 auroral displays one chance arrangement of this kind might be picked out. But during three successive winters, as shown by the above table, there were auroral displays in series with an interval of 27 days plus or minus one day in each case, and with an average interval very slightly exceeding 27 days. The chance of this being merely an acci-

dental arrangement is calculable but is complicated by the fact that there were auroral displays on other days than those in the series. It is sufficient for my purpose to show that the probability of chance occurrence in series of this kind is so small that the more reasonable conclusion is that there is some natural cause for this grouping, and this, I think, is in evidence.

These sequences of auroras at intervals of about 27 days have been noticed before, and have heretofore been attributed to a connection of the aurora with the rotation period of the sun. Fritz in Europe, and Dr. Veeder and Professor Bigelow in this country have each maintained the existence of such a connection, though they differ somewhat in regard to the length of the period. The length of the lunar period formed by the oscillations of the moon to the north and to the south of the earth's equator is also about 27 days, and I was led to investigate the connection of this period with that of the auroras, especially as there seems a physical reason for such a connection. Dr. Nils Ekholm and Svante Arrhenius have shown (Ueber den Einfluss des Mondes auf den electrischen Zustand der Erde, Stockholm, 1894) that the difference of electrical potential between earth and air at the stations in northern Europe is greatest when the moon is south of the equator and least when the moon is north of the equator, while the reverse is true at Cape Horn. Their explanation is that the moon is an electrified body, charged negatively like the earth, and that when it is north of the equator it acts more directly by induction on the parts of the earth beneath, diminishing the electrical potential in the northern hemisphere between the earth and the air, while the potential increases in the southern hemisphere. The reverse takes place when the moon is south of the equator. For the purpose of comparing the positions of the moon north and south of the equator with the occurrence of auroras, I selected from the Nautical Almanac (for the data from which I am indebted to Professor E. C. Pickering) all the dates when the moon reached its greatest northern declination. Taking these as the first day of each period, I classified the numbers of auroras observed in the United States into periods of 27 days. The auroral data was taken from the American Meteorological Journal, vol. ii, p. 227, from the Monthly Weather Review, and from the Proceedings of the Rochester Academy of Science. Table II shows the number of stations in the United States reporting auroras on each day of the lunar period. Columns 1 to 6 show the totals in groups of fifty periods; column 7 shows the totals on each day of the lunar period for the entire interval of 23 years from 1870 to 1892; column 8 gives the departures of the totals of each day from the mean of the column. The results show a very

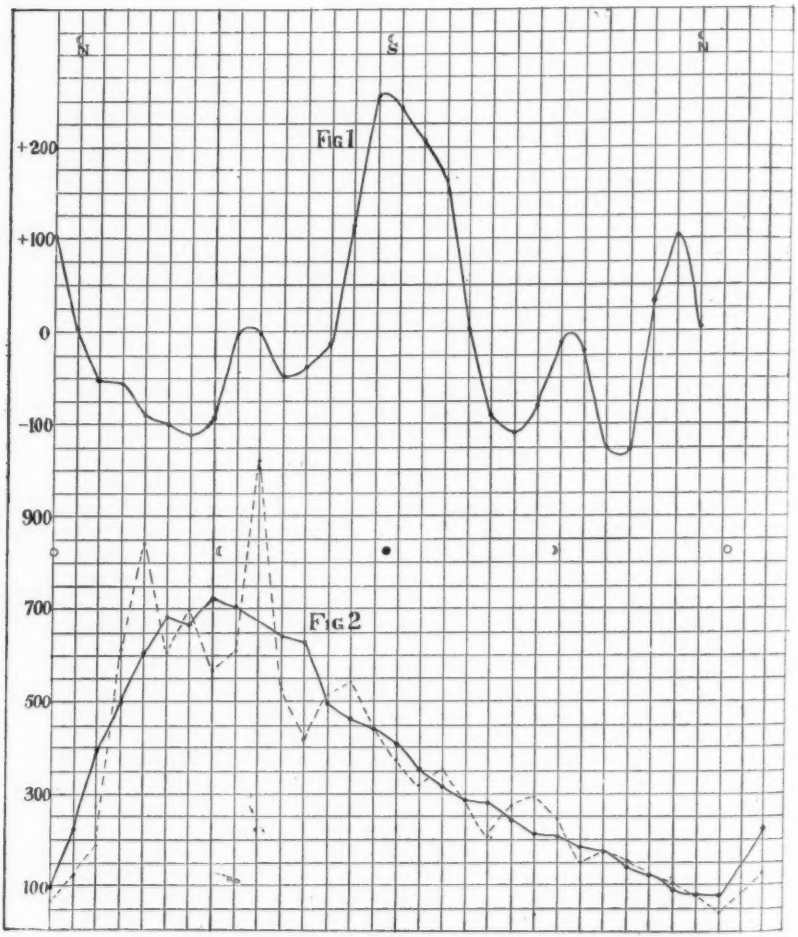
decided excess of the frequency of auroras at the time of the moon's greatest southern declination, and thus agree well with the results of Dr. Ekholm which show the greatest difference in potential between the earth and air in the northern hemisphere at this time. However, the residuals in column 8 show also an excess of auroras about the time of the moon's greatest northern declination and about the time of the moon's passage across the plane of the earth's equator. These excesses, unless accidental, are not easily explained by the theory of differences of potential produced by the induction of the moon. To see

TABLE II.

	1	2	3	4	5	6	7	8	9	10	11	12	13	14
Day of Period.	1870 to 1873	1874 to 1877	1878 to 1881	1881 to 1884	1885 to 1888	1889 to 1892	1870 to 1892	Dept from mean.	Mean of five.	Second order means.	1885 to 1888 Winter half-year.	1893 to 1896 Winter half-year.	1885 to 1888 Summer half-year.	1894 to 1896 Summer half-year.
1	75	77	*117*	188	229	75	761	+217	— 10	+105	25	28	*234*	84
◑ N 2	105	46	97	74	105	49	476	— 68	+ 24	+ 5	17	39	78	116
3	77	60	45	91	113	98	484	— 60	— 10	— 53	11	66	113	107
4	93	132	77	86	50	81	519	— 25	— 75	— 55	8	45	54	147
5	74	65	105	86	26	76	432	—112	— 73	— 90	31	280	13	*215*
6	74	89	74	54	71	70	432	—112	—104	— 98	27	407	35	150
7	50	77	74	80	111	97	489	— 55	— 77	—110	51	310	17	168
8	47	46	56	65	42	72	328	—218	— 57	— 94	24	171	68	126
9	139	149	100	107	93	68	656	+112	— 51	— 2	56	665	23	35
10	121	161	57	91	64	35	529	— 15	— 41	— 1	62	635	14	31
11	88	69	71	112	69	52	461	— 83	— 9	— 46	96	248	11	51
12	115	73	101	89	108	55	541	— 3	— 19	— 36	57	391	34	99
13	89	76	85	44	124	69	487	— 57	+ 62	— 13	66	95	21	33
14	91	210	88	54	118	48	609	+ 65	+116	+115	*154*	257	35	56
15	168	*294*	59	77	*240*	93	*931*	+*387*	+159	+*256*	146	164	69	98
◑ S 16	*221*	115	39	103	124	*128*	730	+186	+*220*	+243	110	85	76	95
17	151	251	77	74	143	61	757	+213	+196	+215	82	80	49	139
18	87	266	72	*233*	93	43	794	+250	+101	+165	57	*690*	39	84
19	71	99	32	93	132	63	490	— 54	+ 38	+ 3	62	140	21	69
20	59	69	49	97	94	86	454	— 90	— 23	— 90	47	173	16	78
21	44	64	51	80	65	115	419	—125	— 73	—109	42	159	34	87
22	127	46	56	90	54	76	449	— 95	— 51	— 79	28	133	106	112
23	71	53	79	166	107	68	544	0	— 69	— 11	38	102	98	173
24	114	57	85	70	139	*133*	598	+ 54	— 83	— 18	8	48	68	106
25	60	39	52	88	86	40	365	—179	— 53	—125	29	48	37	49
26	50	43	27	112	57	60	349	—195	— 9	—128	20	43	52	80
27	69	186	107	85	103	50	600	+ 56	— 30	+ 33	11	39	94	95

what effect smoothing would have on the somewhat irregular numbers in column 8, the mean of each consecutive five was taken and the results are given in column 9. In this column the slight maxima found in column 8 at the time of the moon's equatorial passage are smoothed out, but the maximum at the time of the moon's greatest northern declination is still prominent. However, if instead of taking the mean of five, the

smoothing is done by taking the second order of means, that is, by taking the mean of each two and then the mean of each two of these means, the results when plotted give a



smooth curve without destroying any of the features of the original residuals. The results of this second smoothing are given in column 10 and plotted in the accompanying diagram, fig. 1. The numbering of the days of the period in the table is made to begin one day before the time of greatest

northern declination of the moon. As shown in column 12 the excess of auroras on the fifteenth day, that is at the time of greatest southern declination of the moon, is 46 per cent greater than the mean; while at the times of minima on the seventh and twenty-first days the frequency of auroras is 20 per cent less than the mean. This range of 66 per cent in the frequency of auroras in the totals of three hundred successive periods is entirely too large to be considered accidental.

Each group of auroras of 50 periods given in columns 1 to 6 shows the same general distribution of auroras as the totals. In order to see whether the period remains the same through the different seasons of the year, two groups of data were classified into periods, as described before, and summed for the winter and summer half-year separately. The data for the two groups were taken from tables found arranged conveniently for the purpose, the first by Dr. Veeder in the Proceedings of the Rochester Academy of Science, vol. ii, Plate II, and the other by the editor of the U. S. Monthly Weather Review in that publication. The first covers the four years 1885 to 1888, and the other the three years from October, 1893 to October, 1896. The results are given in columns 11 to 14, Table II. These columns show that there is a strong indication of the reversal of the period between the winter and summer half-years. In the winter half-year the maximum is found near the time of the greatest southern declination of the moon, while in the summer half-year the maximum is found near the time of greatest northern declination of the moon. In attempting to discover the cause of this phenomenon, I noticed that in my table of auroras arranged in periods of 27 days there was a strong drift of the larger numbers to the right, and of such an amount as to indicate a period of about 29 days between them. This period I at once inferred to be the same length as the synodic revolution of the moon, and its cause to lie in the fact that more auroras were seen in the dark of the moon than in the opposite phase. In order to determine to what extent the frequency of reports of auroras was thus affected, the intervals from 1885 to 1888 and from 1894 to 1896 were divided into periods of 30 days, beginning each period with the date of full moon as taken from the Nautical Almanac. The number of stations in the United States reporting auroras on each date was entered in a table in its proper place beginning with the date of full moon, and the totals were obtained for each day of the period during the two intervals mentioned. The results are given in Table III. In column 1 are given the totals for each day of the period from 1885 to 1888, and in column 2 are given the totals for each day of the period from 1894 to 1896. In column 3 are given the totals of

TABLE III.

		1 1885 to 1888.	2 1894 to 1895.	3 Totals.	4 Mean of 5 Days.	5 Second order Means.
◌	1	18	50	68	96	74
	2	8	116	124	226	128
	3	31	157	188	398	302
	4	42	667	709	494	611
	5	39	802	841	609	749
	6	85	521	606	684	688
	7	241	459	700	664	642
◑	8	160	405	565	*720*	610
	9	131	477	608	704	726
	10	118	*1004*	*1122*	648	*845*
	11	175	353	528	641	649
	12	*251*	167	418	628	473
	13	175	354	529	492	504
	14	160	383	543	461	514
	15	187	255	442	442	450
●	16	200	175	375	406	378
	17	140	180	320	355	342
	18	193	158	351	308	327
	19	142	146	288	287	283
	20	119	86	205	282	243
	21	110	162	272	242	261
	22	80	216	296	214	252
◐	23	90	57	147	208	185
	24	48	102	150	184	156
	25	68	109	177	150	163
	26	83	69	152	141	150
	27	55	67	122	122	125
	28	45	58	103	95	98
	29	15	43	58	78	65
◌	30	11	29	40	78	50

the two intervals. The totals in column 3 are smoothed by taking the mean of each consecutive five which appear in column 4, and by taking the second order of means which appear in column 5. The results in every column show the very marked excess in the number of auroras reported between last quarter and new moon. This is the part of the lunation when no moonlight occurs between sunset and midnight, the part of the day when most records of auroras are made, because the observers are then awake. But why there should be a greater number of auroras near the last quarter than near new moon is not very evident. In the column of totals, the frequency of auroras is almost twice as great near last quarter as

near new moon, and in the mean of each consecutive five the maximum number of auroras occurs exactly at the time of last quarter. The irregularities in the totals are almost entirely smoothed out in the column giving the mean of five, and even after they are thus smoothed the number of auroras near the last quarter of the moon is about ten times as great as the number reported near full moon. The results in columns 3 and 4 are shown in the diagram, fig. 2, the lighter broken line representing the unsmoothed totals and the heavy line the mean of five. This great variation in the number of auroras according to the age of the moon furnishes a clue to the reversal of the time of maximum frequency of auroras in the 27-day period, as a result of which the maximum occurs near the time of the moon's greatest northern declination in summer and near the time of the moon's greatest southern declination in winter. In the summer the greatest southern declination of the moon occurs near full moon, so that the chief maximum of auroras occurring at that time is obscured. In the winter the greatest northern declination occurs near full moon, so that the secondary maximum at that time is obscured, and the chief maximum at the time of greatest southern declination is well marked. But as yet there appears no reason why there should be a secondary maximum of auroras at the time of the moon's greatest northern declination, for according to Ekholm's theory of the moon's induction of electricity on the earth, the difference in potential between the earth and the air ought to be at a minimum at the time of the moon's greatest northern declination.

It is well known that when the sun crosses the plane of the earth's equator there is a marked maximum in the number of auroras. This is well shown for the United States by the following table, prepared by Professor Hazen, showing the number of stations reporting auroras during each of the twelve months of the year for an interval of 23 years:

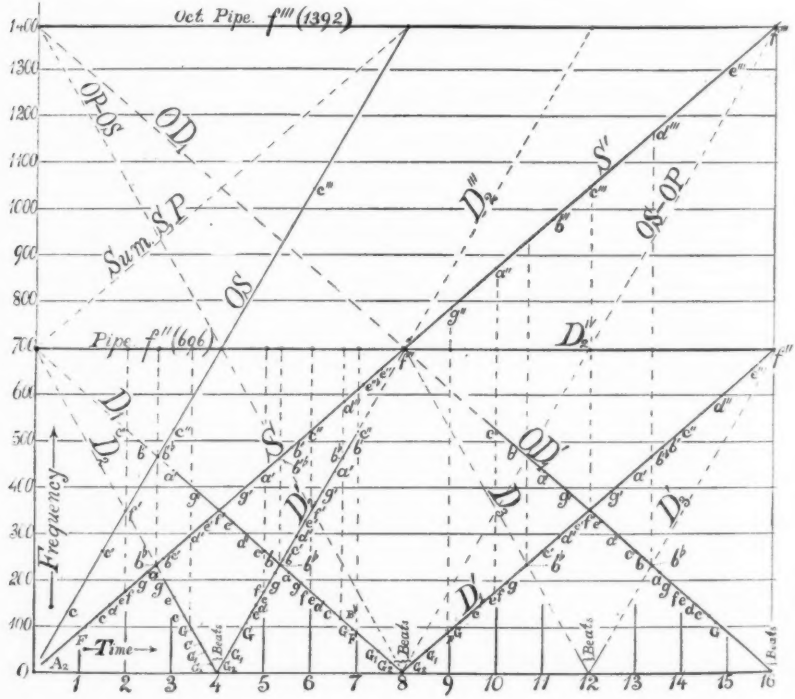
Jan.	Feb.	Mar.	April	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.
1005	1455	1396	*1724*	1270	1061	1223	1210	*1735*	1630	1240	912

The explanation of this, according to the theory of Mr. E. B. Elliot (Bull. Phil. Soc., vol. i, p. 45), is that the number of auroras depends on the rate at which the earth, in its annual orbit, cuts through cosmic lines of electric force.

Since Dr. Ekholm's theory fails to explain the secondary maximum shown by the curves at the time of the moon's greatest northern declination and the tertiary maxima at the time the moon crosses the plane of the earth's equator, I am led to believe that some magnetic effect must be involved, probably engendered by the unequal cutting of the lines of magnetic force by the moon in its motion around the earth.

ART. XIII.—*The Combination Tones of the Siren and an Organ Pipe;* by CARL BARUS.

IN view of the brilliant development of experimental acoustics, I can hardly suppose that the present simple but efficient method of studying combination tones (difference tones in particular) is new. Be this as it may, the subject on its own merits is well worthy of close inspection, and the chart which I submit will, I think, be of much assistance. Without it, the disentanglement of the interpenetrating trains of harmony which reach the listener, is extremely difficult even to a musical ear. I will therefore venture to describe my work.



Helmholtz in his classical experiments obtained his most pronounced combination tones by the aid of his many-voiced siren. Few, however, possess this apparatus. Moreover, in the case given, the difference tones must change from low to high pitch,

and vice versa, at the same time with the siren, supposing that the same musical interval is retained. In order to exhibit the continuity of these phenomena perspicuously (see chart), I have modified this experiment by replacing one siren by an ordinary open organ pipe of fixed and sufficiently high pitch. This is mounted on the same bellows with a simple siren rising continuously from low to high pitch. The advantage gained in this way is best exhibited in the accompanying diagram, but consists essentially in producing difference tones whose frequency-changes vary at first in opposite direction to the pitch of the continually rising siren. Thereafter with a passage through zero, the difference tones change in the same direction as the pitch of the siren. This contrast between ascending and descending cadences, of which there are several successive groups, facilitates recognition, while the apparent scattering of the difference tones into beats beyond the lower limits of audition, and their subsequent emergence and coalescence from beats, is of special acoustic interest. The method is thus peculiarly adapted for demonstrating the whole scope of these complex phenomena even to an indifferently musical ear. It is not so useful in fixing the pitch of a single group of notes, though this also succeeds after a little practice, for an ear accustomed to recognize chords and musical intervals.

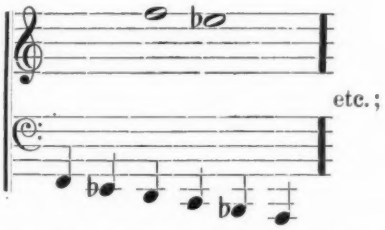
2. To work out the diagram, I used an ordinary open pipe, f''', (frequency $n = 696$) which happened to be available and which was high enough in pitch to admit of difference tones of a wide range below it. The siren was an ordinary small single-voiced König instrument. By supposing the angular velocity of the disc to increase at a small constant rate in the lapse of time—a condition of things nearly enough reached in the experiment—the diagram is constructed by representing the frequencies (n) of the different notes as ordinates, and times, t, in arbitrary units (0 . . . 16) as abscissas. In such a diagram the notes (chords) heard simultaneously will lie on the same vertical. A number of these have been accentuated by drawing the vertical lines in question from note to note, because of their value in orientation. (See chart.)

In the chart the continuous note of the organ pipe (P) is given by the broad horizontal lines at the level $n = 696$, extending quite across the diagram while its first overtone, the octave, is shown by a similar line (OP) at $n = 1392$. The continuity of notes given out by the siren is shown by the broad diagonal (S below f''', S' above f''') extending from the origin at an angle of 45° as far as the height $n = 1392$ or f'''. Beyond this the notes were difficult to produce and their succession too slow. The first overtone of the siren is shown by the line (OS) having double the slope of (S). The successive notes of

the diatonic scale along any of these diagonals are indicated by dots and lettered in the usual way, and these are obviously much more crowded in the lower regions ($n < 696$), where the pitch of the siren changes with proportionate rapidity, than in the upper regions ($n > 696$).

Thus, the first order of difference tones ($D_1 = P - S$)* is shown by the diagonal descending from $n = 696$ and $t = 0$ at an angle of 45° below the horizontal to the abscissa $n = 0$ in the left half of the diagram, and rising from the abscissa $n = 0$, at an angle of 45° to the original level $n = 696$ in the right half of the diagram as shown at D'_1. These are the chief difference tones in question. Where they were heard either directly or indirectly (including beats) the line is drawn in full and the notes indicated. Otherwise the line is broken. Of course it is almost impossible to recognize the precise pitch at which the difference tone is first heard, since the ear is obliged to depend on musical intervals for orientation. With more practice the limits could doubtless be extended.

The interesting feature of this order of difference tones (D_1, D_1'), which are quite sweeping and easily heard, is the transition (at abscissa $t = 8$) of the descending into the ascending cadence.

The difference tone here passes beyond the limits of audition, thereafter to emerge again. With his many-voiced siren Helmholtz used an analogous phenomenon to test these limits. In the present experiment the vanishing of the difference tone is approached by a marked chord of the seventh, $Fc''e''\flat f''$, followed by the effective musical sequence in the key of $B\flat$, which is nearly

finally while the treble is sharpened descending from $F_1 (n = 43)$ to (I think I could say in my case) A_2, where $n = 27$. The last sensations however are much as though the head were being profoundly shaken and the pitch recognized probably imagi-

* The frequency of D_1 is obtained, in other words, by deducting the frequency of the note of the siren from that of the organ pipe, all referred to the same vertical.

nary. Without wishing to enter into the memorable Helmholtz-König controversy, it is certain that the difference tones vanish with the failure of the ear completely, before any beats are heard as such. These introduce an entirely different sensation (by contrast a very commonplace sensation), the sibilant rattling being at first rapid, then slowing up into complete unison, finally to reappear again with excess of frequency in the pitch of the siren over that of the organ pipe. The beats gradually gain in rapidity, finally to vanish in the profound difference tones of the ascending cadence of the first order D_1'. So far as I can make out, the only discontinuity here is physiological and due to the failure of the ear.

The next train of sounds of marked prominence, though not quite so clear and deliberate as the preceding, are the difference tones of the second order ($D_2 = D_1 - S$, or $= OS - P$), due to the combination of the first order of difference tones D_1 with the notes of the ascending siren S_1 as well as to the first overtone of the siren (OS) combining with the organ pipe (P). On the same time-scale, these notes must appear in a descending cadence D_2, vanish through beats, and reappear in an ascending cadence D_2' and D_2''' just twice as rapidly as the difference tones of the first order, as is shown by the greater steepness of the lines converging above $t = 4$ of the abscissa. Beyond this what has been said in general about D_1 in passing through unison applies to D_2. They are strong enough to mask some of the first order of difference tones. They intersect the siren notes (S) and the first order of difference tones (D_1) about in $b^{\flat}$. The latter, coinciding with the $b'^{\flat}$ of the lines D_1 and OS and the f'' of the organ pipe, is easily found. More marked however is the $b^{\flat}$ of D'_2 at its intersection with D_1, coinciding with the $b'^{\flat}$ of S and f'' of the organ pipes. We have here two ascending, one descending and one stationary cadences, and the listening ear hears two orders of difference tones chromatically converge upon the common $b^{\flat}$ and thereafter diverge again. Other coincidences are given in the diagram. Prominent intervals are apt to be in the key of $B^{\flat}$.

In the second half of the diagram ($t > 8$) the second order of difference tones separate into D_2''' ($= OS - P$) and D_2^{IV} ($= S' - D_1'$) coalescing with P at f''.

In the left half of the diagram we have furthermore the octave difference tones OD_1 ($= OP - S$) due to the first overtone of the pipe and the ascending siren, and the summation tones, "*Sum.*" ($= S + P$). Neither of these high trains was I able to hear. Finally the octave or first overtone of both pipe

and siren give a difference tone shown by the line $(OP-OS)$ of the chart. This also escaped me.

I come now to the right half of the diagram ($t > 8$), in which the ascending cadence of difference tones of the first order (D'_1) has already been recognized. Here the siren, S', is continually above f''' and the result is a distinct descending cadence of difference tones of the first order $(OD_1' = OP-S)$, due to the combination of the first overtone, f''', of the organ pipe and the siren. This is the continuation of the train not heard in the preceding section. OD_1' is fainter than D_1, but otherwise like it, finally vanishing into not very distinct beats at $t=16$ of the abscissa. These two first orders of difference tones, as a rule about equally strong, intersect above $t = 12$, in f'', which being an octave below f''' of the organ pipe and heard together with the fifth C'''' of the siren, is a landmark in this region of difference tones. It is upon this note, f'', that the two ascending and descending cadences (D'_1 and OD'_1) chromatically converge, thereupon again to diverge.

But there must be another phenomenon here, namely the difference tones of the third order $(D_3 = OD_1' - D_1'$ and $D_3' = D_1' - OD_1')$ due to the combination of the two difference tones of the first order in this region. With these the difference tone $(D_3 = P-(OS-OP)$ and $(D_3' = (OS-OP)-P)$ would completely coincide. But I have only been able to recognize the third order of difference tones through faint beats at $t = 12$, at which the two steep diagonals D_3 and D_3' converge because of the unison of the component notes in f''. The train $(OS-OP)$ also eluded me. Finally reference to overtones* above the first seemed in every way superfluous.

Brown University, Providence, R. I.

* The relative strength of the different cadences will clearly depend on the relative strengths of the fundamental and overtones. Thus with a pipe b' the cadences of the second half of the diagram came out more clearly, etc.

ART. XIV.—*Some Products found in the Hearth of an Old Furnace upon the dismantling of the Trethellan Tin Works, Truro, Cornwall;** by WM. P. HEADDEN.

THE suite of samples forming the subject of the following notes was presented to the writer by Dr. Richard Pearce of Denver, who states that they were obtained from the hearth of an old furnace in the Trethellan Tin Works at Truro, Cornwall. The furnaces had been idle for a short period prior to the abandonment of the works, which took place about 1872, but they were not dismantled until 1892, and it was at this date that the samples were obtained. These furnaces had been in use for at least sixty, and probably for nearly a hundred years. The ores smelted in them were the usual Cornish tin-ores carrying some arsenopyrite. Dr. Pearce showed, in an article published in 1871, that this arsenopyrite is cobaltiferous, and this is probably the source of the cobalt found in the samples.

Stannous Sulphide.—This compound occurred as black, highly lustrous, monoclinic crystals covering a slag surface to the thickness of nearly a quarter of an inch. The forms made out were 111, $\bar{1}11$, 110 and 010. The crystals are tabular, owing to the development of the clinopinacoid; the lustre is strongly metallic. The crystals are invested with small globules of metallic tin, invisible to the naked eye, but which appear rather abundantly when the crystals are examined under the microscope. The stannous sulphide does not rest immediately upon the slag, but upon a stratum of another crystallized compound, which also appears, but in somewhat isolated groups, through the mass of stannous sulphide crystals.

Crystals which proved to be the same in composition were observed occurring sparingly in the upper part of another mass of crystals an inch, or perhaps more, in thickness; these were much smaller and were quite free from tin. The analytical data on which the identification rests are analyses of these two specimens. In the latter case, 1·78 per cent insoluble has been deducted, and the remainder calculated to a hundred, when the analyses give the following composition for the crystals:

	Per cent.	Atomic Equivalent.		Atomic Ratio.
Sn	71·538	60 63	70·05	1·00
Fe	4·881	8·71		
Cu	0·452	·71		
S	23·129	72·27	72·27	1·03
	100·000			

* Abstract of an article read before the Colorado Scientific Society, Nov. 6, 1897.

The crystals from the slag were contaminated with metallic tin, but I feared to continue the treatment of the mass with ferric chloride until the whole of the tin should be removed, lest the sulphide of tin should be attacked. The analysis of these crystals, soluble in hydrochloric acid, gave:—

	Per cent.
Sn	80·413
Fe	2·905
S	17·134
	100·452

Basing my calculation on the percentage of sulphur found, I make the amount of free tin present 22·91 per cent; deducting this and calculating the rest to one hundred, I obtained:—

	Per cent.	Atomic Equivalent.		Atomic Ratio.
Sn	74·006	62·80	69·53	1·00
Fe	3·769	6·73		
S	22·225	69·43	69·43	1·00
	100·000			

Both analyses lead to the formula SnS, with a small part of the tin replaced by iron. In the first analysis this replacement amounts to 3/22, in the second to 3/28 of the tin.

Iron Arsenide.—This compound was found associated with stannous sulphide in two specimens, in one of them it formed the principal part of the specimen; in the other it formed only a thin layer between the slag and the stannous sulphide, and occurred sparingly throughout the mass of crystals; it also formed the major part of a third specimen.

It was freed from the stannous sulphide by boiling with concentrated hydrochloric acid and subsequently with potassium hydrate. It has a dark steel-gray color. The specific gravity was found to be 7·6500 at 16° C, but this becomes 7·9414 when correction is made for the presence of 1·76 per cent. of sand, specific gravity 2·6. The crystal form is orthorhombic. Most of the crystals show only two forms—110 and 101—but some of them show the additional form, 100, and in one or two cases I observed the presence of a brachydome. The analysis of these crystals resulted as follows:

	Per cent.	Atomic Equivalent.		Atomic Ratio.
Sand	1·760			
As	53·224	70·9		
S	0·536	1·7	74·9	1·00
Sn	2·847	2·3		
Fe	38·304	68·4		
Co	3·640	6·2	74·6	1·00
Ni	Trace			
Cu	Trace			
	100·311			

If we reject the tin and sand, and calculate the remainder to one hundred, the ratio for the arsenic and sulphur to the iron and cobalt is 1:1·03, but I do not think that the tin should be rejected.

This product resembles the mineral loellingite in form, but differs from it in having the formula FeAs. Leucopyrite, etc., are intermediate between these two—$FeAs_2$ and FeAs.

FeAs+SnS.—The next product is a molecular combination of the preceding two: it occurs as black, shining plates belonging to the monoclinic system. The forms recognized on these crystals are the same as those given for the stannous sulphide. The material analyzed was selected with the greatest of care and, while the amount of the substance obtained was small, it was free from crystals of other substances. The analysis resulted as follows:

	Per cent.	Atomic Equivalent.		Atomic Ratio.
Fe	17·949	32·0	35·5	1·06
Co	2·037	3·5		
S	10·671	33·3	33·3	1·00
As	27·166	36·2	36·2	1·08
Sn	43·106	36·5	36·5	1·09
	100·929			

The atomic ratio is evidently 1:1:1:1. Following the indication of the other analyses, it seems that the simplest and probably the correct way of interpreting this ratio is by assigning the formula FeAs+SnS to these crystals; the iron in the iron arsenide being replaced in part, as in the preceding sample, by cobalt.

An Arsenide of Tin, Sn_6As.—One of the samples in the suite was a small mass of bright, highly lustrous crystals of a grayish tin-white color. They were quite brittle, six-sided plates, whose form I could not satisfactorily make out. The crystals have a slight bronze-yellow tarnish. Their composition was found to be:—

	Per cent.	Atomic Equivalent.	Atomic Ratio.
Sn	90·414	76·62	6·02
As	9·441	12·72	1·00
	99·855		

This appears to be a new alloy of tin and arsenic. It is soluble in hydrochloric acid with the evolution of hydrogen arsenide, and the separation of brown flakes, not further examined, except that they were observed to be soluble in nitric acid. Ferric chloride attacked the crystals, taking tin in solution and leaving a bluish black residue containing a nucleus of the original crystal. It was quite impossible to free the crystals from metallic tin, which could be easily recognized with the aid of a magnifier, as minute globules, occurring sparingly on the crystals.

Stannic Oxide: Artificial Wood Tin.—This consisted of an irregular mass weighing about one and a half pounds. There was a central portion of metallic tin running lengthwise through the mass: at certain points this had entirely disappeared, but its former presence was shown by the structure of the mass. The oxide has a banded structure, and the banding is at some points plainly concentric. The hardness varies greatly: some portions of the mass have a hardness quite equal to that of the natural wood tin, while others are quite soft. The oxide was reduced in hydrogen to determine the oxygen, and the resulting metal dissolved in hydric chloride for further analysis. The result was as follows:

	Per cent.	Atomic Equivalent.		Atomic Ratio.
Sn	76·969	65·2		
Cu	0·240	0·38	65·9	1·00
Fe	0·091	0·2		
Co	0·060	0·1		
O	22·550	140·9	140·9	2·13
As	Trace.			
	99·910			

The manner in which these products may have been formed, the length of time required to produce them, etc., as well as the conditions which obtained at the time of their production, are, unfortunately, almost wholly matters of theory. The formation of the stannic oxide was evidently by oxidation of the metallic tin, a portion of which still remains, but whether aqueous vapor and hot gases, other than the air, had part in its formation is an open question. The arsenide may have been formed in a magma of molten tin, and the tin may have subsequently been removed by a process of liquation. There are some indications that this is really what happened, but the extreme cleanness of the crystals is very remarkable if this was the case, the angles throughout the network of crystals being sharp and not filled up with tin, as one would expect. In regard to the stannous sulphide and iron arsenide, I think that their occurrence on the slag is quite suggestive that they have been deposited from an atmosphere furnishing the conditions necessary for their formation. The principal part of the arsenide was deposited first and subsequently the sulphide, but some of the arsenide was deposited both subsequently to and simultaneously with the sulphide, as the crystals of the arsenide occur implanted upon those of the sulphide, and when the atmosphere, from which the crystallization took place, furnished the compounds in molecular equivalents, they crystallized together, giving the compound FeAs+SnS. In the masses furnishing the arsenide, there is neither any residual tin recognizable as such, nor sign of flowing, nor yet of absorption by the bricks of the hearth.

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ART. XV.—*Kant as a Natural Philosopher;* by GEO. F. BECKER.

KANT'S fame as a metaphysician has completely overshadowed his reputation as a physicist, at least outside of Germany. One occasionally sees a slight reference to the fact that he had an idea of some sort of nebular hypothesis, but usually the context conveys the impression that it was only a futile dream. Thus a well known historian of astronomy stated in 1882 that Kant had put forward "a true nebular cosmogony, though one in which the primitive reign of chaos was little likely to terminate." In 1869, however, Huxley wrote a few vigorous paragraphs on Kant's results* and shortly afterwards Lord Kelvin, referring to Huxley's abstract, remarked that Kant's paper only wanted the knowledge of thermodynamics "to lead to a thoroughly definite explanation of all that is known regarding the present actions and temperatures of the sun and other heavenly bodies."†

All of Kant's earlier papers were on physical subjects and he retained his interest in natural philosophy through life. He studied Newton: at one time he lectured on mathematics, and his course on physical geography was repeated through many years. Some of his papers are now of little interest, but others, at least from an historical point of view, are very important. The titles of those which seem to me most noteworthy are given in a footnote below.‡

The first three of these papers, which are also the most original, were all published before Kant's poverty allowed him

* Presidential Address, Geol. Soc. London, Discourses Biological and Geological, 1894, p. 320.

† Address to Geol. Soc., Glasgow, April 5, 1869.

‡ The volume and page numbers refer to Immanuel Kant's Sämmtliche Werke, edited by G. Hartenstein, Leipzig, Voss., 1868.

1754. Untersuchung der Frage, ob die Erde in ihrer Umdrehung um die Achse, wodurch sie die Abwechselung des Tages und der Nacht hervorbringt, einige Veränderung seit den ersten Zeiten ihres Ursprungs erlitten habe, vol. i, pp. 179–186.

1754. Die Frage; ob die Erde veralte, physikalisch erwogen, vol. i, pp. 187–206.

1755. Allgemeine Naturgeschichte und Theorie des Himmels, oder Versuch von der Verfassung und dem mechanischen Ursprunge des ganzen Weltgebäudes nach Newton'schen Grundsätzen abgehandelt, vol. i, pp. 207–345.

1756. Neue Anmerkungen zur Erläuterung der Theorie der Winde, vol. i, pp. 473–487.

1763. Der einzig mögliche Beweisgrund zu einer Demonstration des Daseins Gottes. Siebente Betrachtung, Kosmogonie, vol. ii, pp. 180–193.

1785. Ueber die Vulcane im Monde, vol. iv, pp. 193–202.

1786. Metaphysische Anfangsgründe der Naturwissenschaft, vol. iv, pp. 355–462.

1802. Physische Geographie. Auf Verlangen des Verfassers aus seiner Handschrift herausgegeben und zum Theil bearbeitet von Dr. Fr. Th. Rink, vol. viii, pp. 145–452.

to complete his studies for the degree of Doctor of Philosophy. The paper on the volcanoes of the moon gave him an opportunity of reasserting his belief in his nebular hypothesis after many years of reflection, and of making a very great addition to the cosmogonic apparatus. The discussion of the metaphysical bases of natural science contains interesting reflections and definitions. It is difficult to say how much of it was absolutely new, since all natural philosophers have dealt with the same ideas. His definitions of fluids and solids* sound extremely modern. "A material in which a motive force, however small, produces shear is a fluid, no matter how powerful the cohesion of its parts." A solid is a body "which resists shear with a certain degree of force." "The resistance to shearing is die Reibung," by which he means what is now denoted by rigidity; liquids, he says, have none of it. In his theory of the winds Kant was anticipated by Geo. Hadley,† but he supposed himself to be the first to perceive the effect of the rotation of the earth on northerly or southerly winds. He deduced from this effect the cause of the easterly trades, the westerly winds of latitude 28° to 40° and the monsoons. Kant's paper on this subject was very probably intended as a reply to D'Alembert, whose theory of the winds procured him the prize of the Berlin Academy in 1746. D'Alembert endeavored to account for atmospheric currents as an immediate result of the attraction of the sun and moon. The physical geography was edited from lecture notes by a pupil when Kant was an old and broken man. There is good reason to suppose that it would have been more valuable if Kant had written it out himself at an earlier date.

Kant was indeed no dreamer, but one of the best informed men of his time. It was the great object of his life to discourage visionary speculations, "Schwärmerei," and to reduce all subjects to the confines of reason. His theory of the heavens, however it may appear to modern readers, was conceived from this point of view. Even Newton had felt himself called upon to attribute various details of the solar system to the direct intervention of the Creator, and it was the habit of that day, as of other days, to confound the mysterious with the miraculous. Kant endeavored to offer a rational explanation of many mysteries and in large measure he was successful. He saw, as well as one can see now, that astronomy has two divisions. In spherical and gravitational astronomy mathematics reigns supreme, and certainty is attainable. In physical and historical astronomy, on the other hand, a high degree of probability is the nearest possible approach to absolute truth. Kant says of his theory of the heavens: "In general,

* Kant's Werke, vol. iv, p. 420. † Phil. Trans., vol. xxxix, 1735, p. 58.

the highest geometrical precision and mathematical infallibility can never be demanded from a treatise of this description. If the system is founded on analogies and agreements, developed according to the rules of credibility and in a logical manner, it satisfies the conditions which its object demands." In judging of Kant's theory, allowance must be made for the faulty data at his disposal, particularly in respect to the masses of the planets, and for the undeveloped state of astronomical science. I know of no more striking way to emphasize the remoteness of the period when the theory was published than to note that in 1755 Lagrange was 19 years old, William Herschel 17, and Laplace 6. Kant was not always right, and indeed he himself threw doubt on certain of his results, but some of his great successors fell into some of the same pitfalls. That many of his theories are held to-day shows the keenness of his scientific faculty, and almost excites a regret that he did not remain loyal to Urania. It is doubtful, however, whether he could have been restrained from metaphysical inquiries. There is a small group of men, naturalists by instinct, who seem irresistibly compelled to include among the phenomena forming the object of their inquiries the mechanism of the intellect and the relations of ideas. Such are Aristotle, Bacon, Descartes, Kant and a living English philosopher.

Kant was not the first philosopher to speculate on the origin of the heavenly bodies, though he was the first Newtonian to do so. If one leaves aside some vague speculations of the ancients, there can be no doubt that the first germ of the nebular hypothesis is to be found in Descartes' Principles of Philosophy, published in 1644 (43 years before Newton's Principia). It may briefly and imperfectly be represented as follows. The content of the universe is divisible into three categories.* The first corresponds nearly to the luminiferous ether of the present day and its agitation produces light, the second answers approximately to gas, the third to solid or liquid substances. This last Descartes calls "opaque matter." In the beginning these materials were almost uniformly distributed through space and possessed motion in nearly equal amounts.† The nebulous mass was divided into patches of common rotation, or the famous Cartesian *vortices*. In each vortex there were subordinate vorticules, eddies in a whirlpool. The motion of rotation was most rapid at the center, dimin-

* Les principes de la Philosophie, Cousin's edition. 1824, part 3, sec. 52. The three categories are summarized as "être lumineux, être transparent, et être opaque ou obscur."

† In all. "Autant de mouvement qu'il y en a encore à présent dans le monde." (Part 3, sec. 46). Descartes supposed the momentum of the universe a fixed quantity. Leibnitz, I believe, was the first to assert the constancy of the total energy.

ished with increasing radius to a minimum, and increased again towards the edge. Nebulous material was driven from the center towards the periphery in the equatorial plane of the vortex and flowed back again along the axis of rotation. The first stage in the evolution of systems was the segregation of opaque matter in spots, such as are still to be seen on the sun. In the smaller subordinate vortices these accumulated until they formed a crust which partially or completely arrested the vertical circulation. The minor stellar bodies so consolidated were then captured by the larger vortices and were incorporated into them as passive constituents. These opaque masses sought a position of equilibrium in the rotating fluid of the vortex. At first such a body would descend towards the center. If, however, it possessed sufficient "solidity," which seems to be mass per unit of exterior surface,* it soon acquired such centrifugal velocity as to be thrown out of the periphery of the vortex, passing into the next similar vortex and so on. It thus became a comet. If the opaque mass had less solidity, it found a position of equilibrium within the vortex and became a planet moving on an orbit of small eccentricity.† The planets of smallest solidity are nearest the sun.‡

Swedenborg also published a vortical cosmogony in 1734, forty-seven years after the appearance of Newton's Principia. Messrs. M. Nyrén§ and E. S. Holden† have each given some

* Cf. part 3, sec. 122. Descartes regarded space as a "plenum" and does not here consider motion in a vacuum. The measure of force was solidity into velocity. The resistance of the plenum was proportional to the exterior surface of a moving mass.

† Et parceque toutes les petites parties de la matière qui compose un tourbillon ne sont pas égales ni en agitation, ni en grandeur, et que leur mouvement est plus lent selon qu'elles sont plus éloignées de la circonférence, jusques à un certain endroit au-dessous duquel elles se meuvent plus vite, et sont plus petites selon qu'elles sont plus proches du centre, ainsi qu'il a été dit ci-dessus, si cet astre est si solide que, devant que d'être descendu jusques à l'endroit où sont les parties du tourbillon que se meuvent le plus lentement de toutes, il ait acquis autant d'agitation qu'en ont celles entre lesquelles il se trouvera, il ne descendra point plus bas vers le centre de ce tourbillon, mais, au contraire il montera vers sa circonférence, puis passera de là dans un autre, et ainsi sera changé en une comete. Au lieu que s'il n'est pas assez solide pour acquérir tant d'agitation, et que pour ce sujet il descende plus bas que l'endroit ou les parties du tourbillon se meuvent le moins vite, il arrivera jusques à quelque autre endroit entre celui-ci et le centre, où étant parvenu il ne fera plus que suivre le cours de la matière qui tourne autour de ce centre, sans monter ni descendre davantage, et alors il sera changé en une planète. Part 3, sec. 119.

‡ Therefore the earth must be less dense than Mars, which has the smaller diameter (part 3, sec 147), and therefore the earth must consist of a rigid shell with a fiery interior (part 4, sec 3), Descartes concludes. This is the first assertion that the earth has a fluid interior. Descartes was driven to make it by the exigencies of a false theory. The doctrine once accepted, its origin was forgotten: and but for Lord Kelvin, it would probably have remained a misleading article of faith with geologists to the end of the 19th century.

§ Vierteljahrsschrift der astronomischen Gesellschaft, 1879, p. 80. This paper is translated in the New Church Review, July, 1897.

† North American Review, vol. cxxxi, 1880, p. 377.

account of this theory. The solar system, according to Swedenborg, was formed from a chaotic mass which rolled together to a great sphere and afterwards, by its rotation, threw off a ring. This by expansion burst, and the fragments shrank to planets. I see no important advantage in this as compared with the Cartesian cosmogony. Even if deductions from any such vortical theory were correct, they could still have no scientific standing, since they would be conclusions from false premises and their correctness therefore accidental.

In 1750 Thomas Wright (of Durham) published a book, "The Universe and the Stars," to an abstract of which Kant expresses obligations. It is so tiresome as to seem much longer than it is, and the only original conclusion which I can find is that the visible stellar system is spheroidal or cylindrical, the Milky Way marking the position of the periphery. At the center is "an intelligent principle" round which the system revolves. Wright, like Cassini, thought that Saturn's rings were composed of small satellites,* a conclusion which he draws from the eccentricity of the rings.

Turning now to Kant's cosmogony, it may be interesting to note in the briefest terms the more striking of his views in a connected way, and then to draw attention to such of them as seem anticipations of the opinions held by subsequent investigators.

In developing his nebular hypothesis Kant regards the solar system from two points of view. The first contemplates the unity of plan. Six planets and nine satellites revolve in orbits about the sun, all moving in the same sense, that namely in which the controlling central body, the sun, rotates. Their orbits vary but little from a single plane, that of the sun's equator; the more distant bodies belonging to the solar system (viz: comets), however, show divergencies standing in sufficiently close relation to the deficiency of the impressed movement. This unity of plan indicates a single pervading cause of motion. On the other hand, from the second point of view, the interplanetary space is substantially empty; it contains no matter through and by which accordant motions could have been communicated to the system.†

Hence this space must once have contained matter, and the material abstracted from interplanetary space must be that which is now gathered in the members of the solar system; for the quantity of matter in the universe is inalterable. Kant therefore assumes that the materials of the solar system were

*I have only seen the American reprint (without plates). Phila., Wetherill, 1837. This edition has amusing notes of the mystic order by C. Wetherill and C. S. Rafinesque.

† Kant's Werke, vol. i, p 245.

originally disseminated in an elementary state, as atoms, throughout the entire space belonging to the system. The material is not supposed to be heated or in motion, the average density of the nebula is many times smaller than that of the terrestrial atmosphere, and the conditions are explicitly the simplest conceivable.*

He proceeds to develop from this hypothesis not merely the conditions of the solar system but of the star cluster bounded by the Milky Way and other star clusters which he supposes to be represented by the nebulæ. As the solar system is to the cluster of which it is an insignificant member, so is this to a greater group of clusters; and infinite space is occupied by a rotating group of similar constitution but of an infinite order of magnitude and of complexity.†

For the solar system he deduces many remarkable conclusions. The shrinking nebula acquires rotary motion, he erroneously supposed, in consequence of irregular impact of the component particles and their elasticity.‡ Tendencies to motions in all directions, excepting in one resultant plane, are suppressed by mutual interferences of the free particles. Most of the material accumulates at the center, in the sun, but a wide, thin disc of heterogeneous matter remains. This disc consists of discrete particles each of which has acquired such a velocity and direction as to maintain the appropriate orbital motion. On the whole the inner zones of the disc will contain a larger proportion of denser substances than are to be found in the outer ones; while the outer rings, being of relatively large circumference, will be of greater mass than the inner ones. Mutual attraction and adhesion, beginning at relatively massive particles, cause the agglomeration of the particles in any zone or ring to single planets or to groups of planetary bodies. The direction of axial rotation is deduced from the mode of formation, the outer part of any accumulating planetary mass moving with greater velocity than the inner portion.§ (The proof offered for this last proposition is unsatisfactory.)‖

* Kant's Werke, vol. i. p. 247. † Kant's Werke, vol. i, p. 290.

‡ Though Kepler's law of areas was well known to Kant, he did not realize the more general theorem, that the moment of momentum of a system on which no external forces act is constant.

§ Kant's Werke, vol. i. pp. 267 and 258.

‖ Mr. H. Faye (L'origine du monde, 1884. pp. 117 and 143) rejects Laplace's nebular hypothesis on the ground that the latter is in error in his deduction as to the rotation of the planets. Mr. Faye thinks that because the inner numbers of a swarm of particles tend to revolve with a higher velocity than the outer ones, retrograde rotation would ensue. This is a case where simple explanations are tempting but misleading. The problem rigorously dealt with involves the solution of the problems of three or more bodies. It does not appear to me that Mr. Faye has given any satisfactory proof of his position. The same may be said of Laplace, but it may be suspected that this great man had solved some particularly

Kant appeals to his theory of Saturn as a confirmation of the credibility of his whole hypothesis. Nevertheless he represents the rings not as a relic of a condensing nebula but as an emanation from the planet. The rings, according to his theory, are composed of discrete particles* condensed from fumes and vapors which had been thrown off from the equator of the planet by rapid rotation while the planet was still in a highly heated condition. These particles acquired their moment of momentum from the planet and revolved in obedience to the law that this moment is constant. He attempts to account for the stability of the mass by its division into many rings, pointing out that if it were continuous, friction must destroy its motion.

Kant also computed the then unknown period of rotation of Saturn on the hypothesis that centrifugal force and attraction are equal at its surface, so that the rings are practically continuous with the planet. He obtained a period of $6^h\ 23^m\ 53^s$. Herschel in 1794 from direct observations found its period $10^h\ 16^m$, which differs from Prof. A. Hall's determination by a very few minutes. Mr. Faye supposes the German astronomers to have omitted mention of Kant's prediction out of regard for the philosopher, the discrepancy between it and the observations being "overwhelming." The French astronomer, however, also remarks that the discrepancy, while calling for explanation, does not invalidate the theory. Perhaps Kant's theory of tidal retardation would have filled the gap. Kant also speculated on the flattening of Saturn and concluded that the density must increase towards the center of the planet. This conclusion he extended to Jupiter and inferred its applicability to other heavenly bodies.

In the zodiacal light he saw an analogue to the Saturnian rings, but he drew an essential distinction between the materials which reflects the zodiacal light and the rings of Saturn. The velocity of rotation of the sun being quite insufficient to maintain particles in free revolution near its surface, the heat of the sun must coöperate to keep the fame-like mass in a state of expansion even if the material came from the sun, as Kant thought probable. He also contemplated the possibility that it is a remnant of the nebula.

simple cases, applicable to the question in hand, of the problem of three bodies and generalized from the result. It seems impossible that he should not have perceived the true nature of the question, or that he should have committed himself without an inquiry which was qualitatively satisfactory.

* Kant reached this result independently, but afterwards became acquainted with an investigation of Dominico Cassini's, Mem. de l'Acad., Paris, 1705, p. 14, in which the rings of Saturn are regarded as swarms of minute satellites. This is nearly but not quite Kant's view; for his rings are, so to speak, raw material of which the satellites might have been made, but were not.

The planets in agglomerating become fluid. Newton, and indeed Descartes, had previously pointed out that planets had passed through the fluid condition. Kant seemingly ascribed the fusion of planets and satellites to the impact of their component parts.*

Kant expressed in the clearest manner and with sufficient demonstration his view that the period of rotation of the moon was reduced to coincidence with that of its revolution by tidal action on the satellite while it was still liquid. He draws the conclusion that the moon is somewhat younger than the earth.† He also demonstrates that oceanic tides retard the rotation of the earth, and predicts that the earth will eventually turn one side only to the moon.

In a cooling planet, according to Kant, there is a rough and partial separation by density. Such a process is still progressing in Jupiter which Kant regards as still fluid. The planets eventually become solid throughout.‡ The obliquity of the axes of rotation to the planes of revolution may be in part due to changes in shape accompanying consolidation and in part due to irregular distribution of velocities in the gathering mass of a planet. The progressively increasing density of the planets as the sun is approached is due to the varying proportion of the heavier elements in the contracting nebula; the lighter elements being deflected into orbital paths more easily than the heavier ones. Kant in 1755 regarded the sun as a solid burning mass, attributing its small density to the fact

* Kant's Werke, vol. i, p. 302.

† Ibid., p. 183 to 186. It may be of interest to translate a few sentences literally from this paper, "Ob die Erde in ihrer Umdrehung um die Achse einige Veränderung erlitten habe." "The water of the ocean covers at least one-third (sic) of its surface and is kept in continual motion by the attraction of the above mentioned celestial bodies [the sun and moon]. Moreover this motion is in one direction, exactly opposed to the rotation. . . . . Since this flow is opposed to the rotation of the earth, we have here a cause which may be counted upon to retard and diminish rotation continuously to the extent of its capacity . . . . The termination of this change of rotation will occur when the earth's surface, from the point of view of the moon, shall be relatively at rest, i. e. when it rotates in the same time as the moon revolves. If it were fluid throughout, the moon's attraction would soon bring its rotation down to this fixed remainder. This immediately reveals the cause which has compelled the moon in its journey round the earth always to expose to it the same aspect. . . . The attraction which the earth exerts on the moon, acting on the satellite while it was still fluid, must have reduced the rotation of the moon (formerly no doubt greater than now) to this fixed residue in the manner just explained."

‡ This is distinctly stated in Allgemeine Naturgeschichte; and in Ob die Erde veralte. One of the sentences in the preceding footnote is equivalent to an argument that, were the earth not solid, internal tidal friction would long since have reduced its period of rotation to that of the moon's revolution. This ingenious mode of reasoning on the earth's rigidity has not been revived or quantitatively tested so far as I know. It is worthy of consideration. In the Physische Geographie, 1802, however, he asserts that while the surface irregularities of the earth show solidity of the superficial portion, it does not follow that time enough has yet elapsed to carry consolidation to the center.

that the greater part of the material of the nebula acquired by impacts only a very small tangential velocity, and thus fell directly into the central mass. This fallacious explanation arose from his incorrect view of the generation of moment of momentum.

Further light as to the origin of the heat of stellar bodies came to Kant in 1785, thirty years after the publication of his theory of the heavens. Adair Crawford, in 1779, published his experiments on the development of heat by the compression of gases. These discoveries Kant says indicate a method of making comprehensible the development of any degree of heat as an accompaniment of the formation of heavenly bodies (suns or planets) from the initial nebula. He points out that the increase of temperature depends upon the initial and final densities and upon the brevity of the time of condensation. He infers that the central body, on account of its greatest mass and attraction, must develop the greatest heat and is rendered capable of being a sun.*

Comets in Kant's system are derived from the extreme portions of the nebulous mass which gave rise to the solar system. The original position of the cometary matter being at so vast a distance from the center, gravitation acted but feebly; elastic impact of the gravitating material was therefore also feeble; the mass acquired but little tangential velocity and the resulting orbits show correspondingly great eccentricity. The feebleness of attraction and the tenuity of the mass at the exterior of the nebula may account for the independence of the elliptic manifested by comets; but Kant expresses his own dissatisfaction with this explanation. Retrograde comets Kant hardly attempts to explain; he even thinks there may be an optical illusion.† Kant supposed the eccentricity of the planets to increase with distance from the sun, and predicted that planets with greater eccentricity than Saturn and at greater distances from the sun would be discovered.‡ These in his opinion would form in a sense a transition to cometary bodies.

Kant considers the end of the solar system as well as its inception. The world itself is doomed to destruction as a habitable planet by the process of base-levelling and consequent flooding by the ocean.§ All the planetary bodies will eventually fall into the sun, the inner ones first, in consequence of the universal tendency of motion to gradual retardation. While Kant had a perfectly clear idea of the retardation of the motion

* Ueber die Vulcane im Monde 1785: Kant's Werke, vol iv, p. 201.

† Kant's Werke, vol. i. p. 265.

‡ Ibid., p. 243.

§ The process of base-levelling is excellently described in the paper "Ob die Erde veralte?" 1754, Kant's Werke, vol. i, p 203.

of rotation by tidal action, and inferred the former more rapid rotation of the moon, I do not find in his papers a definite statement as to the means by which the dissipation of orbital energy is achieved. He did not regard planetary space as a perfect vacuum and may have relied upon the resistance of the interplanetary medium for the predicted effect. The fall of such great and numerous masses as the planets into the sun would, he says, immeasurably increase the heat of the central body. He also seems to have regarded the planets of small density as capable of combustion. The heat thus generated he believed great enough to dissociate the material of the system and to restore it to its original nebulosity.* Then commences a fresh cycle of evolution.

Kant's doctrine of the restoration of the nebula by the dissociation attending the collapse of the solar system must certainly be pronounced false in the light of the second law of thermodynamics. It is almost impossible, however, to see how Kant could have avoided this error before Carnot's day. His attempt was to frame a theory for the universe, for all time, and some regenerative principle was a necessity to such a scheme. The second law of thermodynamics as now understood seems to divide everlasting time into two portions, the former of which is a perfect blank. In the light of present knowledge it would appear that the entire stellar system, planets and residual nebulæ alike, must have been in the condition of an immensely diffused nebula not more than a few hundred million years ago and that the universe is now hastening towards eternal death. It is evident that the initial diffusion, whatever its date and its degree, can have lasted but an instant, during which the potentialized molar energy represented the maximum proportion of the total energy of the universe. By the principle of dissipation, molar energy is continually wasting away or undergoing conversion into molecular energy. Hence if the quantity of matter and the total energy of the entire system are constant, the molar energy at a period preceding that instant must have been smaller and must then have increased. The only alternative compatible with the second law seems to be to suppose that the total energy of the universe underwent an increase at that time. The universe would then not be a conservative system, the equation of energy would not apply to it, and aliquid ex nihilo fit. Such an increase of energy might or might not be due to a creation of matter. Thus if the molar energy was maximum within a finite period, either the quantity of energy in the universe is variable and has been increased or there is an undiscovered, regenerative, third law of thermodynamics

* Kant's Werke, vol. i, p. 302.

which in some circumstances supersedes the second. In either event the number of the fundamental principles of natural philosophy would be still incomplete.

It is possible, however, to imagine the date of this critical period in the universe, the epoch of initial nebulosity, infinitely remote, though little intellectual satisfaction is to be attained by this means. If a strictly finite amount of matter were distributed in a space not merely relatively large but absolutely infinite, so that every atom should be at a truly infinite distance from any other, then a strictly infinite lapse of time would be required to produce a finite degree of condensation. Known laws would therefore account for present conditions if the ratio of space to mass in the universe is not merely large, but (like time itself) absolutely infinite. Such an assumption however seems preposterous, while it is far from incredible that some natural law remains to be discovered, even one which would make a *perpetuum mobile* of the universe as a whole.

The attempt which Kant made to create moment of momentum from the impact of nebulous matter is curiously out of accord with the rest of his investigation. In treating of Saturn's rings he avoided any such mistake, and the persistency of moment of momenta follows from his own scheme of the universe. When the solar system collapses, only to be restored to a nebulous state, as he supposed, it will be in rapid rotation, according to his own theory. It is thus only for the very first of the infinite succession of developments that his exposition asserts a fallacious source for rotational movement.

Kant seems to have anticipated Laplace almost completely in the more essential portions of the nebular hypothesis. The great Frenchman was a child when Kant's theory was issued, and the Systéme du Monde, which closes with the nebular hypothesis, did not appear till 1796. Laplace, like Kant, infers unity of origin for the members of the solar system from the similarity of their movements, the small obliquity and small excentricity of the orbits of either planets or satellites.* Only a fluid extending throughout the solar system could have produced such a result. He is led to conclude that the atmosphere of the sun, in virtue of excessive heat, originally extended beyond the solar system and gradually shrank to its present limits. This nebula was endowed with the moment of momentum which Kant tried to develop by collisions. Planets formed from zones of vapor, which on breaking agglomerated. He appeals to the rings of Saturn as an illustration of nebular contraction; he also considers the Zodiacal light as due to a

* The retrograde satellites of Uranus were discovered by Herschel in 1787, but Laplace in his hypothesis does not refer to them.

nebulous remnant. In the first editions of the Systéme du Monde, Laplace, like Kant, supposed some of the nebulæ to represent stellar islands outside of the Milky Way system. In 1824 the work was revised and this explanation was omitted. In the revised edition, but not in the earlier ones, Laplace refers the equality of the moon's periods of rotation and revolution to tidal action caused by the earth's attraction in the still fluid moon.* Comets were regarded by Laplace as little nebulas formed outside of the solar system, while Kant considered them as arising in the extreme portions of the solar nebula.

The main points of comparison between Kant and Laplace seem to be these. Kant begins with a cold, stationary nebula which, however, becomes hot by compression and at its first regenesis would be in a state of rotation. It is with a hot, rotating nebula that Laplace starts, without any attempt to account for the heat. Kant supposes annular zones of freely revolving nebulous matter to gather together by attraction during condensation of the nebula. Laplace supposes rings left behind by the cooling of the nebula to agglomerate in the same way as Kant had done. While both appeal to the rings of Saturn as an example of the hypothesis, neither explains satisfactorily why the planetary rings are not as stable as those of Saturn. Both assert that the positive rotation of the planets is a necessary consequence of agglomeration, but neither is sufficiently explicit. The genesis of satellites is for each of them a repetition on a small scale of the formation of the system. Each refers comets to nebulous matter more distant than the planets, but Kant thought it merely the superficial portion of the solar nebula. Both contemplated extragalactic systems of stars.

While Laplace assigns no cause for the heat which he ascribes to his nebula, Lord Kelvin goes further back and supposes a cold nebula consisting of separate atoms or of meteoric stones, initially possessed of a resultant moment of momentum equal or superior to that of the solar system. Collision at the center will reduce them to a vapor which then expanding far beyond Neptune's orbit will give a nebula such as Laplace postulates.† Thus Kelvin goes back to the same initial condition as Kant, excepting that Kant endeavored (of course vainly) to develop a moment of momentum for his system from collisions.

The extragalactic stellar systems imagined by Kant were

* This seems to have been overlooked, for Lord Kelvin (Geological Time, Geol. Soc. Glasgow, Feb 27, 1869) ascribes the discovery to Helmholtz

† On the Sun's heat. Lecture to Royal Inst., 1887, or Popular Lectures, vol. i. p. 421.

similar to those which the elder Herschel regarded as probable in the earlier part of his career, and in which Laplace believed prior to 1824. The subject has been under discussion ever since and is not definitely settled. Of late years the application of the spectroscope and the researches of Proctor have made it seem possible that only one stellar system is visible. If others exist they may be too far off for their light to reach the earth, or have progressed too far towards extinction to be sensibly luminous.*

After Kant's explanation of the heat of the sun as due to compression, the theory does not seem to have been revived until Helmholtz enunciated it in 1854.† It now has the adherence of Lord Kelvin, and, so far as I know, of all physicists as the main source of solar radiation.

The novelty of Kant's view of Saturn's rings was that he supposed them to be composed of fume, or minute discrete particles, instead of satellites, as Cassini and Wright had done. It is rather strange that Laplace, though, like Kant, appealing to Saturn as an illustration of the nebular hypothesis, should yet have regarded the rings as solid. The older view, practically in the Kantian form, was reviewed by Maxwell,‡ who was evidently unaware of the earlier hypothesis.

Little advance has been made in explanation of the Zodiacal light since Kant's time. It is regarded as partly gaseous and partly composed of solid matter, perhaps meteoroids. Its spectrum seems to be that of reflected light.

Kant's idea of a transition from planets to comets has not received verification, and the orbit of Neptune is at present the most nearly circular of all the planetary orbits, excepting that of Venus, instead of the most eccentric as Kant would have supposed. Hence his prediction that planets exterior to Saturn would be found cannot be placed to his credit, its truth being accidental.

Kant was not the first to notice the decrease in density of the planets Earth, Mars, Jupiter and Saturn, while the densities of Mercury and Venus were unknown in his time. Such

* The possibility that ether envelopes a stellar system like an atmosphere without extending indefinitely into space, is perhaps worth considering in reference to the hypothesis of the existence of distant star groups.

† Helmholtz's paper, Ueber die Wechselwirkung der Naturkräfte, was a popular lecture delivered in Königsberg and, according to Thomson and Tait, was on the occasion of a commemoration of Kant (Nat. Phil., vol. ii, p. 493). In this paper appeared also his views on the tidal retardation of the earth and the moon, and the collapse of the solar system. Kant is mentioned as author of the nebular hypothesis, but not in connection with these more detailed questions. It is certainly most curious that at a Kant celebration Helmholtz should have announced the rediscovery of four of Kant's theories.

‡ Proceedings R.S. Edin., vol. iv, 1858, and Adams prize essay, for 1856. Both may be found in his collected works.

merit as he possessed in the matter is consequently confined to the attempt to account for the greater frequency of relatively dense matter among the inner planetary rings. Euler supposed the densities to be proportional to the square roots of the mean motions. If a is the mean distance, this is equivalent to making the density proportional to $1/a^{3/4}$. Lagrange in 1782* assumed the density to be proportional to $1/a$. In this hypothesis Lagrange was anticipated by Buffon† in 1769.

Modern results do not indicate such simplicity. Since there is compression in the inner portions of a planetary body, mean density must be a function of mass. This is perhaps a sufficient explanation of the fact that Earth has a higher average density than Mercury, Venus and Mars. The planetary densities are divisible into two groups. Those planets just named do not differ very greatly in this respect. The outer group from Jupiter to Neptune are vastly less dense, but it is not certain that the diameters of these bodies represent anything except the exterior of their atmospheres. They are probably still highly heated, and their average density is not very different from that of the sun. Now the sun is certainly vastly hotter and more expanded than the outer planets, and it must therefore be composed of material which would be denser than the mass of Jupiter at the same temperature. It is conceivable that this difference is due solely to the greater pressure within the sun's mass.

Kant's opinion (1755) that Jupiter is still fluid seems to be the earliest expression of that view. Buffon in 1778 announced his opinion that both Jupiter and Saturn are fluid.‡

The theory of base-levelling has been so generally accepted by geologists and physical geographers throughout the century that it is scarcely worth while to mention individuals, but some of those who do not happen to be familiar with Kant may be glad to read a few of his vigorous sentences on the subject. "In respect to change of the earth's shape there remains to be discussed a single cause which can be reckoned upon with certainty; it consists in the fact that the rain and the streams continually attack the land and sluice it down from the highlands to the lowlands, gradually making the elevations into plains and, so far as in them lies, strive to rob the globe of its inequalities. The action is certain and no matter of opinion. The land is also subject to this action so long as there is material on the

* Nouveaux Mém. Acad. Berlin, 1782, p. 153.

† Oeuvres comp Paris, 1827, vol. i, p. 215.

‡ Époques de la Nature. Oeuvres compfètes de Buffon, Paris, 1827, vol. v, p. 84. Miss A. M. Clerke in her History of Astr. in the 19th Century, 2d edition, errs as to the date of Kant's publication on this subject and assigns the priority to Buffon.

declivities which can be attacked and transported by rain water."*

In 1754, according to Kant, the Berlin Academy offered a prize for a solution of the question "Whether the rotation of the earth which produces the alternation of day and night, has undergone any change since the time of its origin? What is the cause and how can the fact be established?" † Kant's paper printed in the same year dealt with both the tidal retardation of the moon while still in a fluid state, and the retardation of the earth's rotation by the marine tides. The subject seems then to have fallen into complete oblivion for nearly seventy years. The retardation of the moon's rotation was rediscovered by Laplace in 1824. The earth's retardation was maintained by Prof. James Thomson in conversation about 1840, as Lord Kelvin mentions.‡ J. R. Mayer expressed the same opinion in print in 1848.§ Helmholtz in 1854 discussed the tidal retardation both of the moon and of the earth.‖ William Ferrel and C. Delaunay afterwards again suggested tidal retardation of the earth. It was only at a later date that Kant's paper was brought to light.

So far as I know, J. J. von Littrow, in 1830, was the first to revive the Kantian idea of a final collapse of the solar system.¶ In 1854 Helmholtz again recurred to it. He explains the future annihilation of the rotation of the planets by tidal friction, and relies on a certain inevitable amount of resistance in interplanetary space to destroy orbital motion. Thomson and Tait, in 1867, showed that after the earth has come to expose to the moon a constant aspect, the effect of the solar tides will be to reduce the distance between earth and moon until they come together. By a similar argument it is said to follow that loss of energy (irrespective of a resisting medium) will not cease until all the bodies of the solar system "subside into a state of motion in circles round an axis passing through their center of inertia, like parts of one rigid body."** It does

* Kant's Werke, vol. i, p. 203.

† I find in the memoirs of the Academy no mention of this prize, and Kant's paper printed in 1754 does not seem to have been offered in the competition. Peschel and Leipoldt, however, refer to it as having taken the prize. (Physische Erdkunde, vol. i, 1884, p. 54.)

‡ Geological Time, Address Geol. Soc. Glasgow, Feb. 27, 1868.

§ Beiträge zur Dynamik des Himmels, Heilbronn, 1848. Translated in Phil. Mag., vol. xxv, 1863, p. 403.

‖ Populärwissenschaftliche Vorträge, 2d Fascicle, p. 130.

¶ In Vorlesungen über Astronomie, second part 1830, p. 146, he announces this approaching catastrophe and ascribes it to external influences acting on the system. Later he indicated friction in a resisting medium as the external influence. This explanation probably appeared in the first edition of his work, Die Wunder des Himmels, 1834.

** Nat. Phil., 2d ed., sec. 276. The text of this section is the same as in the first edition of 1867.

not seem certain that Kepler's laws are sufficiently rigorous for application to ensuing conditions, but if they are so, the number of bodies would seemingly reduce to two. By the third law, if T is the time of revolution and a the mean distance, T^2/a^3 is the same constant for all the planets. Hence if the solar system reaches a condition in which T is the same for all the planets, a must also be the same, and all the planets must revolve on the same circle. In view of Lagrange's investigations on the stability of the solar system, it hardly seems possible that all the planets should acquire and permanently pursue the same exactly circular orbit unless they were to coalesce to a single mass.

In his preface to his Theory of the Heavens, Kant says: "I seek to evolve the present state of the universe from the simplest condition of nature by means of mechanical laws alone." After more than one hundred and forty years of rapid progress in science, it cannot be denied that his attempt was astonishingly successful.

Washington, D. C., December, 1897.

ART. XVI.—*The Islands and Coral Reefs of the Fiji Group;* by ALEXANDER AGASSIZ.*

. . . . On our arrival at Suva we found the "Yaralla," a twin-screw steamer of about 500 tons, chartered from the Australasian United Steam Navigation Company, awaiting us. We have now been about six weeks cruising among the islands, and have found the boat admirably suited for our purpose. During the past summer I had shipped to Australia our outfit for dredging, sounding and for deep-sea towing as well as all the materials necessary for preserving our collections. This equipment we found safely stored in the "Yaralla." I had also brought with me deep-sea tow-nets of the various patterns used by the Prince of Monaco, by Dr. Giesbrecht of the Naples Zoölogical Station, and by Hensen on the "National" Expedition, in order to compare their efficiency with the Tanner deep-sea self-closing net in use on the "Albatross" and which I had adopted on my various expeditions. Unfortunately our time here is so limited and the conditions for towing at great depths among so many intercepting islands are such that the results likely to be obtained seemed to make it unadvisable to devote the time necessary for such a comparison. Although nothing was done to test the different deep-sea self-closing towing nets, yet we have made at various points along our course a number of deep hauls, from 150 fathoms to the surface, with the open net. A large number of hauls were made off Suva which gave us an excellent collection of pelagic forms, the collections of Crustacea and of Acalephs being specially noteworthy. Mr. Mayer will prepare a report on the Acalephs. When practicable we have also made collections on the reef-flats of various islands and atolls.†

I also brought to Suva a complete diamond-drill boring apparatus and a competent man to superintend the work: Mr. W. Eyers, recommended to us by the Sullivan Machine Co., of Chicago, from whom the apparatus was obtained.‡ To provide against contingencies, a comparatively small hand machine was sent, capable of drilling to a depth of from four to five

* Extracts from a letter of Alexander Agassiz, dated Suva, Fiji Islands, December 15th, 1897, addressed to E. S. Dana.

† Dr. Wm. McM. Woolworth and A. G. Mayer accompanied me as assistants. Dr. Woolworth and my son Maximilian have taken a large number of photographs illustrating the physiognomy of the islands and their reefs. A selection from these I shall use in my final report.

‡ I have to thank the Trustees of the Bache Fund of the National Academy of Sciences at Washington for an appropriation of $1200 towards defraying a part of the expenses of boring.

hundred feet. An oil motor was also provided to expedite the work with increasing depth.

This machinery had already been shipped when information reached the United States that Professor David of the University of Sydney had left for the Atoll of Funafuti in charge of an expedition to take up the unfinished work of boring of the party in charge of Professor Sollas, sent out by the Council of the Royal Society of London. The day before leaving Cambridge for the Pacific, news arrived that Professor David's party had succeeded in reaching a depth of nearly 600 feet, the bottom still being in coral. This information seemed to settle the coral question, and all I hoped to accomplish was merely to confirm the work of Professor David by boring in some other district. Subsequent information received from Professor David leads me to think that the matter is not so simple as was represented by the newspapers. From what we have seen thus far of the Fiji islands reef I can only conclude that the boring at Funafuti has settled nothing and that we are still as far as ever from having a general theory of the formation of coral reefs. In fact with the present information obtained here I should never have thought of boring in the atolls of this group, for reasons which will be given presently.

The track which we followed was so arranged as to include in our first trip one or two of each type of island and of the different types of atolls, barrier and fringing reefs in the group. Starting from Suva, after visiting Mbenga we went to Ovalau, Wakaya, Makongai, Koro, skirted along the western shore of Taviuni, examined the northeastern coast of the same island, passed through the Matangi passage to Motua Levu and Motua Lailai, skirted along the western extremity of the Nanuku reefs. From there we steamed to Wailangalala, where we landed our boring apparatus and the crew needed for working the same. We then turned north, passing close to Nuku Mbesanga and Adolphus reef and entered Ngele Levu Lagoon. We next examined the Ringgold islands, paying special attention to Thombia, an extinct crater in Budd reef. From there we returned to our former anchorage off Thurston Point in Taviuni and followed our old track back to Wailangalala, where we found our boring party settled and at work. We then steamed south, examining Williamson reef, the Kimbombo islets, Bell reef and entered the Vanua Mbalavu Lagoon through the Ngillangillah passage, leaving the lagoon by the Tonga pass. We touched at Mango, Tavutha, Naiau and Lakemba, passed Aiwa, entered the Oneata Lagoon, visited Thakau Lekaleka, touched at Motha, entered the Komo Lagoon, the Yangasa cluster and the Ongea Lagoon. We passed Fulanga, close to the entrance, which was too shallow to allow

our vessel to enter, but near enough to give an excellent idea of its characteristic structure. We next touched at Kambara, anchored in the crater of Totoya, stopped at Moala: from there we made Solo Light House, examined the North Astrolabe reef, steamed through the Great Astrolabe reef, coming out west of Ono. We next examined a part of the shore of Kandavu and returned to Suva by way of Vatu Leile. On our second trip we visited Ngau, Nairai, the Horseshoe reef, Mbatiki and entering the Moturiki channel south of Ovalau, examined the barrier reef between it and Suva as far as Mbau. After our return to Suva we made a third trip along the southern coast of Viti Levu as far as Naudronga; skirting the reef as closely as was prudent, we were able to follow the changes in the Great Barrier reef of Viti Levu west of Suva as it gradually passes into a fringing reef and disappears off the Singatoka River, to appear again first as a fringing reef and then as a barrier reef extending beyond the Nandi waters to the west of Naudronga. We then paid a second visit to Vatu Leile and returned to Suva, having steamed a little over 1300 miles.

I came to Fiji under the impression that we were to visit a characteristic area of subsidence; for according to Dana and Darwin there is no coral reef region in which it is a simpler matter to follow the various steps of the subsidence which has taken place here. Dana, in his last discussion of the coral reef question, states that it is impossible to find a better series of islands than those of the Fiji to illustrate the gradual changes brought about by subsidence, which transform a volcanic island with a fringing reef to one with a barrier, and next to one with a circular reef ring and finally to one in which the interior island has disappeared and has left only a more or less circular reefing. For these reasons one of the Fiji atolls promised to be an admirable location for boring and settling the question of the thickness of the coral reef of an atoll. My surprise was great, therefore, to find within a mile from Suva an elevated reef about 50 feet thick and 120 feet above the level of the sea, the base of the reef being underlaid by what is locally called soapstone, probably a kind of stratified volcanic mud. The western extension of this reef can be traced at several points along the north shore of the harbor of Suva; the island of Lambeka and Vua and Dra-ni-mbotu, which are from sixty to ninety feet in elevation, being part of an elevated reef extending to low-water mark. It was this elevated reef or its extension westward which we traced from the Singatoka river to the Nandi waters. A short distance inland from the mouth of the Singatoka there is a bluff of about 250 feet in height, composed of a coral-reef limestone which is the inner exten-

sion of the elevated reef patches and bluffs visible on the shore of Viti Levu. I am informed by Dr. Corney that the islands of Viwa and Asawailau to the northward of the Nandi waters are also remnants of this elevated reef.

But the traces of extensive elevation are not limited to the larger island of Viti Levu. I found the islands on the rim of the atoll of Ngele Levu to consist entirely of coral rock elevated to a height of over sixty feet on the larger island. The surface of the island where we crossed it was a mass of hummocks of honeycombed, potted and eroded coral-masses resembling in every way the elevated reefs with which I had become familiar in the Bahamas, Cuba and Florida. The northern sides of the island of Ngele Levu are on the very outer edge of the rim of the lagoon, deep water running up to the shore line. We next found that at Vanua Mbalavu the northern line of islands were parts of an elevated reef forming vertical bluffs of coral-rock which had been raised by the central volcanic mass of the main island to a height of over 500 feet at Ngillangillah, at Avea to 600 feet, at the Savu Islands to 230 feet and on the main island to a height of nearly 600 feet. On the south of the main island the elevated coral bluffs are very much lower, those of Malatta and Susni reaching a height of a little over 400 feet. Going farther west and south we find at Mango the vertical bluffs of an elevated coral reef of over 600 feet and underlaid by volcanic rocks which crop out at the sea level.

At Tavutha the coral limestone bluffs are probably 800 feet high. At Naiau they are over 500 feet, at Lakemba they reach a height of about 250 feet on the southwest side of the island; the rest of the island is volcanic. On the island of Aiwa the elevated reef is fully 200 feet thick. In the Oneata group the highest point of the elevated reef bluffs is about 160 feet. South of the volcanic island of Motha and enclosed within the same barrier reef, on the island of Karoni the elevated reef is about 120 feet thick. On the three islands of the Yangasa group it attains a thickness varying between 240 and 390 feet, and finally on Ongea, the most southeasterly cluster we visited, the elevated reef attains a thickness of nearly 300 feet. At Fulanga the elevated reef attains a thickness of 360 feet, at Kambara it is about 200 feet thick, and at Wangawa it is perhaps over 300 feet: these islands are in part volcanic. Finally at Vatu Leile, the most westerly island we examined, the elevated reef forming the island is 110 feet thick at its northern extremity.

All this plainly shows that the southern part of Viti Levu and as far south as Vatu Leile, and the whole length of the windward islands of the Fiji group from Ngele Levu on the

north to Ongea on the south, have been subject to an elevation of at least 800 feet; as there is abundant proof that a great part of the thickness of the elevated reef has been eroded to reduce it in certain localities to the level of the sea or to leave at others bluffs and islands or islets, the occurrence of which we have traced at so many points.

But the evidence of a very considerable elevation is not limited to that furnished by the remains of the elevated reefs just mentioned: it is natural to assume that the elevation we have just traced was but a part of a more general elevation which perhaps took place in late Tertiary times and in which the whole group was involved. It is plain that there must have been most extensive denudation and erosion going on throughout the group for a very considerable period of time, geologically speaking. The outlines of the islands deeply furrowed by gorges and valleys, the sharp and serrated ridges separating them, the fantastic outlines of the peaks of Viti Levu, Vanua Levu and Ovalau, all attest to the great work of atmospheric agencies which has been going on for so long. The separation of islands, islets or isolated rocks from the points or spurs of the larger islands also bears witness to the great length of time during which action of the sea necessary to bring about their separation has been at work: adding to this the fact that we are in a region of a former powerful and extensive volcanic activity, the traces of which can still be seen in all directions, forces which have undoubtedly played a great part in the lifting of the island masses and their subsequent shaping to their present outlines.

From this evidence I am inclined to think that the corals of to-day have actually played no part in the shaping of the circular or irregular atolls scattered among the Fiji islands; furthermore that they have had nothing to do in our time with the building up of the barrier reefs surrounding either wholly or in part some of the islands; I also believe that their modifying influence has been entirely limited in the present epoch to the formation of fringing reefs, and that the recent corals living upon the reefs either of the atolls or of the barriers form only a crust of very moderate thickness upon the underlying base. This base may be either a flat of an eroded elevated reef or of a similar substructure of volcanic rocks, the nature of that base depending absolutely upon its character when elevated in a former period to a greater height than it now occupies.

Denudation and erosion act of course more rapidly upon the elevated reef rocks than upon those of a volcanic character. It is therefore natural to find that the larger islands like Kandavu, Taviuni and Ovalau are of volcanic origin, while the

islands which once occupied the area of the lagoons of Ngele Levu, of the Nanuku reef, of Vanua Mbalavu, of the Argo reefs, of the Oneata, Yangasa, Aiwa, Ongea and Vatu Leile clusters being elevated coral reefs, they have disappeared almost entirely, leaving only here and there a small island to attest to the former existence of the more extensive elevated reef once covering the whole area of what is now an atoll. Smaller volcanic islands like Matuku, Moala, Ngau, Nairai and Koro also show the extent to which each island has been eroded after its elevation. The erosion being least in Koro and Matuku, somewhat greater in Moala and Ngau and still greater in Nairai. In such atolls enclosing volcanic islands like Mbenga, Wakaya, Makongai, the erosion and denudation have been still greater, these islands covering but a comparatively small part of the area once occupied by the island originally covering the area of the lagoon. Denudation and erosion have been still more active in the Ringgold islands, in the Kimbobo cluster and in Komo, and it may have gone so far as to leave no trace in an atoll to indicate either its volcanic or coral (elevated reef) origin: the shape of the atoll being entirely due to mechanical action and not being connected in any way with the growth of the corals which have been found afooting upon reef-flats formed by atmospheric agencies or by the action of the sea.

So that as far as we can judge from the case of the Fiji islands, the shape of the atolls and of the barrier reefs is due to causes which have acted during a period preceding our own. The islands of the whole group have been elevated, and since their elevation have, like the northern part of Queensland, remained nearly stationary and exposed to great and prolonged denudation and erosion, which has reduced the islands to their present height; the platforms upon which the barrier-reef corals have grown being merely the flats left by the denudation and erosion of a central island of greater size than that now left; while the atolls are similar flats from the interior of which the islands have been eroded and the lagoons of which have been continually scoured by the action of the sea, the incessant rollers pouring a huge mass of water into the lagoon, which finds its way out through the passages leading into it.

In the Fiji islands the atolls and islands, or islets, surrounded in part or wholly by barrier reefs, have not been formed by the subsidence and disappearance of this central island, as is claimed by Dana and Darwin. The Fiji islands are not situated, as was supposed, in an area of subsidence, but on the contrary they are in an area of elevation, so that the theory of Darwin and of Dana is not applicable to the islands and atolls of the Fiji group.

What the age of the elevated reef of the Fiji is, I am unable to state; its aspect and position show it to be of considerable age, probably antecedent to the present period. In many ways it resembles some of the late Tertiary elevated limestones which I have seen on the northern and southern coasts of Cuba. The great thickness which the elevated coral reefs attain in this group, at least 800 feet, also shows that they may have been deposited originally during a period of subsidence, but not a period of subsidence taking place in our epoch or which could have had any effect in shaping the outline of the islands of the Fiji group and their accompanying reefs.

Whether the elevation of the Fiji group corresponds in time with that of northern Queensland, I am unable to state. I can only suggest that it is not improbable that the elevation of Queensland and of the Pacific islands to the east, New Caledonia, the Loyalty Islands, the Solomon, New Hebrides including Fiji and Samoa, may have been synchronous. It may be that these islands have, like northern Queensland, been subject to an immense erosion and denudation which have reduced them to their present proportions.

The elevation may have been preceded, as in Queensland, in still earlier geological times by a great period of depression, during which the thick beds of coral reef limestone may have been formed. How far east this elevation extended is not known; its area probably included the Cook islands and Tahiti, and judging from some photographs I should feel inclined to consider atolls of the Paumotus as having been formed by causes similar to those which shaped those of the Fijis.

The evidence thus far collected on the Fijis shows the futility of boring in this group. Any result obtained would merely at some point indicate the thickness of a former elevated reef; a reef formed in a period preceding our own. We should obtain information which could have no bearing on the main question, if I am correct in the interpretation of what I have observed, information in fact which may be obtained as one steams along without the trouble or cost of boring. Should I be correct, it would be natural to look upon the results of the boring at Funafuti much in the same light and to assume that the island, as well as others in the Ellice group, is also in this area of elevation and that the great thickness of coral obtained was reached by boring in the base of an ancient reef. So that the results obtained by Professor David from the boring at Funafuti do not assist us in any way in corroborating the theory of subsidence as essential to the formation of atolls.

However that may be, it only emphasizes what has been said so often, that there is no general theory of the formation of

coral reefs, either barrier, or atolls, of universal application. Each district must be examined by itself,—at least such has been my experience in Florida, in the Bermudas, the Bahamas, in Cuba and the West India Islands and the Sandwich Islands. The results of this trip show plainly that the theory of Darwin and Dana, of the formation of atolls and of barrier reefs by subsidence, is not applicable to the Fiji islands, notwithstanding the boring at Funafuti. In all the localities I have visited the coral reefs form but a thin crust upon the underlying base (it is not more than 50 to 60 feet thick in Florida), and the shape and slope of this base is in no way due to the growth of the corals living upon it.

This still leaves open the question of the formation of such thick masses of coral-reef rock, which though they may originally have been formed by subsidence, as other massive deposits have been, yet may also have been formed by the gradual pushing out to seaward of the outer edge of a reef; the reef increasing both in height (depth) and in width by the constant pushing out of the mass of débris and of blocks detached from the outer edge, forming a talus upon which corals may grow whenever the talus has reached the depth at which they thrive. I am inclined to think that the careful study of such a shore reef will alone give us a correct idea of the manner in which such thick masses of coralline limestone may have been formed.

There is still another phase in the formation of atolls which has received but little attention. I refer to the formation of atolls as the result of the denudation and erosion of volcanic summits or of extinct craters. There are in the Fiji two extinct craters which are most interesting: one of these is the small extinct crater of Thombia on the Ringgold islands. The highest point of its rim, the exterior circumference of which is about two miles, is nearly 600 feet, and it is continuous with the exception of a small part of its eastern edge, about a fifth of a mile, across which reaches a fringing reef, the extension of the fringing reef surrounding the island. This reef closes the entrance into the crater, which is about half a mile across at the level of the sea and has a depth of 24 fathoms. The other extinct crater is that of the island of Totoya, an isolated peak in the southern part of the group. It is about six miles in outer diameter with an inner basin of three miles and a depth of 34 fathoms. The highest point of the rim is 1200 feet and at two points it is low, forming in one case a narrow isthmus separating the crater from the outer lagoon. The horns of the open rim are connected by a fringing reef-flat on which thunders the Pacific swell, piling up the water into the great basin of the crater. This water finds its way out through an opening called the "Gullet," which though narrow forms an excellent passage to the anchorage inside of the crater. Totoya

has not only a fringing reef but also a barrier reef somewhat triangular in shape, surrounding the island. It is evident that the barrier reef has been formed upon the denuded and eroded spurs of the island which once extended seaward from the outer rim of the volcano.

Supposing now that the erosion of both Thombia and Totoya had continued long enough to reduce the rim of these volcanoes to the level of the sea or to form a chain of small islands, we should have, as soon as corals had covered the flats thus formed, which indicate the former existence of the rim, atolls of nearly circular form. The one, that of Thombia, being quite small with a circumference of 2 miles and a depth of 24 fathoms without patches in the central lagoon; the other being much larger, more than 25 miles in circumference, having a depth of 34 fathoms inside the lagoon. The lagoon of the Totoya atoll would be dotted with patches some of which formed parts of the rim, others being the remains of eroded spurs extending towards the center of the extinct crater.

There are in Fiji a number of small atolls from one to three or more miles in circumference the formation of which, it seems to me, can only be satisfactorily explained on the theory that they have been formed upon the eroded summits or rims of extinct craters, the rim of the volcano having been eroded either to a continuous flat or to flats separated by deeper passages (as in the case of the low parts of the rim of Totoya) forming entrances into the enclosed lagoons. Such atolls are Motua Levu, Motua Lailai, the Adolphus reef, Bell reef, Williamson, Pitman and the Horseshoe reefs and Thakau Lakaleka. Of course it is possible that some of these atolls may have been formed from the erosion and denudation of isolated peaks or ridges. It is also possible that some of the larger atolls in which are enclosed volcanic islands, like Vanua Mbalavu, Komo, Motha, Lakemba, Mbenga, the Ringgold islands and others, may represent parts of the rim or ridges and spurs of volcanic peaks and extinct craters which have disappeared by erosion and have left the outer flats upon which the barrier-reef corals have grown; while the deeper valleys and gorges of these now eroded volcanic islands represent the undulations in depths of the lagoons. The depths inside the lagoons vary greatly: in the case of Vanua Mbalavu we find 72 fathoms on parts of the eastern slope of the lagoon. These great depths, far beyond any at which corals can grow, represent the elevated gorges and slopes of the volcanic peaks which probably once extended over the whole area enclosed by the outer reef, during the elevation of which the reef which covered a part of the same area was lifted to its present or even to a greater height.

Such large volcanic centers with extensive craters of considerable depth are not unknown. We can reconstruct conditions from Totoya which would give us an atoll open to the west, with a few islands on the outer rim and a greatest depth of 250 fathoms inside the lagoon. Again Haleakala in the Sandwich islands has a crater with a depth of nearly 250 fathoms, while many small volcanic peaks, some fully 1200 feet in height, rise from its bottom. The diameter of Haleakala is fully as great as that of any of the atolls in Fiji. So that the great depth of the lagoons of some of the atolls can no longer be considered as a proof of the theory of subsidence.

Fulanga is also interesting as illustrating the formation of an atoll by the same causes which have produced the Sounds in the Bermudas. In the case of Fulanga we have a volcanic summit which has raised the elevated coral reef forming its rim to a height of probably 200 feet. This coral rim has been broken through and the action of the sea has gradually hollowed out in the interior a circular sound resembling a crater, which is due solely to the disintegration of the inner parts of the elevated reef. In the interior and outer edge of the Sound of Fulanga many parts of this reef still exist as small mushroom-shaped islands or small rounded or conical hills. It is also probable that some of the elevated reef-flats forming the rims of atolls owe their origin to causes similar to those which have shaped the crater-like Sound of Fulanga. This has perhaps been the case with such atolls as Ngele Levu, the Oneata, Ongea and Yangasa clusters and others.

The great variety of causes which have been active in shaping the present physiognomy of the reefs and atolls of Fiji shows the impossibility of assigning any one factor, like subsidence for instance, as is done by Dana and Darwin, as the single cause for the formation of the many different kinds of atolls and barrier-reef islands to be found in the Fiji group. The formation of the great barrier reef of the southern shores of Viti Levu is due to causes very similar to those which have given to the northern coast of Cuba between Nuevitas and Matanzas its present physiognomy. Along those parts of the island where denudation and erosion proceed rapidly owing to the soft character of the shore rocks, very extensive flats have been formed as those south of Ovalau. When the reef-barrier flats have been eroded from a harder base, like volcanic rocks, the flats are less prominent and somewhat more extensive when the old elevated coral reef formed the shore hills; or the reef-flats may disappear altogether when the harder volcanic rocks have been only little affected by erosion or denudation. From the nature of the negro-heads scattered upon the reef-flats it is generally a simple matter to ascertain the character of the base of the reef-flats of an atoll or of a barrier reef.

We were fortunate in being at Levuka at the time of the appearance of the "Bololo." On the morning of the 17th of November we left the ship at 3 o'clock bound for a spot named "Bololo" Point, about 3 miles south of Levuka. We had scarcely reached the spot when our guide fished up a few of the worms and in a few minutes the water was full of them. Canoes put off from the shore; men, women and children were wading on the reef with nets and all kinds of utensils to catch "Bololo." With the increase of daylight the "Bololo" became more abundant and at one time they were so plentiful that the water surrounding the boat was filled with them to such an extent as to resemble thick vermicelli soup. We made an excellent collection of the worms, preserving a large number by different methods. We found, as we had expected, that their sudden appearance was connected with spawning. There were males and females full of sperm and eggs. When in captivity the discharged spermatozoa rendered the water milky and the masses of eggs were left as patches of dark green granules on the bottom of the dish. The discharge of the eggs and spermatozoa was followed by the collapse of the worms, of which nothing was left but an empty skin scarcely visible. The "Bololo" seems thus suddenly to disappear. The males are light brown or greenish, while the females are colored dark green. Their activity while swimming about is something wonderful and the bursting of the animal when it discharges its eggs or spermatozoa is quite a peculiar phenomenon. Dr. Woodworth has made it an object to collect all the material possible in regard to the "Bololo," and on our return he will prepare a paper on this interesting annelid.

I have to thank the State Department at Washington for their kind offices in obtaining for us letters from the Foreign Office to the Government of Fiji. Sir George O'Brien, the High Commissioner for the western Pacific, gave us all possible facilities for visiting the different islands of the group. I am also indebted to the Hon. Charles Stuart, the Colonial Secretary, and to the Hon. W. L. Allardyce, assistant native Commissioner, for assistance and advice. To Dr. Corney and the Hon. John Berry I owe information regarding the presence of elevated reefs at various points I had not examined. To Captain D. Calder I am greatly indebted for his interest in our behalf. We were fortunate in retaining the services of Captain Robert Cocks as pilot during our expedition. His knowledge of the reefs is accurate and extensive. Finally I have to thank Captain Thomson as well as the officers and crew of the "Yarralla," who have been indefatigable in our interest.

I hope during the coming summer to prepare a fully illustrated report of this interesting cruise. . . . . .

Art. XVII.—*Simple Compensated Shunt Box;* by Wilbur M. Stine.

The usual method to compensate for the shunting of a galvanometer that the resistance of the circuit may remain unaffected by the introduction of the shunt, is to insert in series with the galvanometer in succession certain auxiliary or compensating resistances.

If the resistances of the various shunts to the galvanometer be denoted by $S_1, S_2, S_3, \ldots$, the multiplying factors by $n_1, n_2, n_3, \ldots$, taken in the order of their descending values, and the compensating resistance by $r_1, r_2, r_3, \ldots\ldots$: a set of defining equations may be given to the usual method for compensation. These equations are, for a galvanometer of resistance, g,

$$S_1 = \frac{g}{n_1 - 1} \tag{1}$$

$$S_2 = g\,\frac{n_2(n_1-1)}{n_1(n_2-1)^2} \tag{2}$$

$$S_3 = g\,\frac{n_3(n_1-1)}{n_1(n_3-1)^2} \tag{3}$$

$$r_1 = g\,\frac{(n_1-n_2)}{n_1(n_2-1)} \tag{4}$$

$$r_1 + r_2 = g\,\frac{(n_1-n_3)}{n_1(n_3-1)} \tag{5}$$

and

$$r_1 + r_2 + r_3 = g\,\frac{n_1-1}{n_1} \tag{6}$$

According to the usual practice, n_1, n_2, n_3 have the respective values, 1000, 100 and 10, and the preceding equations readily reduce to constant relations, thus—

$$S_1 = g \times 0{\cdot}001001$$
$$S_2 = g \times 0{\cdot}010193$$
$$S_3 = g \times 0{\cdot}1233$$

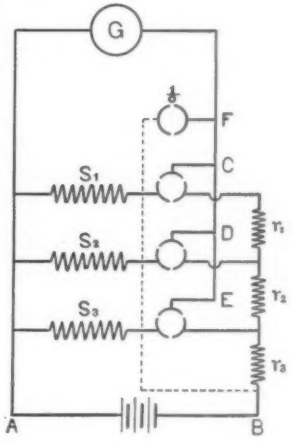
and

$$r_1 = g \times 0{\cdot}00909$$
$$r_2 = g \times 0{\cdot}100909$$
$$r_3 = g \times 0{\cdot}889$$

The foregoing equations are obtained only by long and tedious algebraic processes, and it is readily apparent that their final values are such as to throw doubt on the accuracy of the adjustment of a box.

On inspection it is seen that both the structural and analytical difficulties inherent in the usual form of compensated shunts arise from the fact that the compensating resistances are introduced into the circuit to be shunted.

By re-arranging the connections and attaching them to three part sockets, a marked simplicity is introduced into the calculations for a given set of shunts.



Proceeding to investigate the new arrangement of the resistances and shunts clearly shown in the accompanying figure,—the resistance of the galvanometer circuit through shunting becomes successively R_{c_1}, R_{c_2}, R_{c_3}, and in general

$$R_c = \frac{g}{n_x} \qquad (7)$$

Now write

$$\left.\begin{aligned} Q_1 &= r_1 + r_2 + r_3 \\ Q_2 &= r_2 + r_3 \\ Q_3 &= r_3 \end{aligned}\right\} \qquad (8)$$

For the shunt at C, the compensating resistance Q_1 is

$$Q_1 = g - \frac{g}{n_1} = g\,\frac{n_1 - 1}{n_1} \qquad (9)$$

Similarly

$$Q_2 = g\,\frac{n_2 - 1}{n_2} \qquad (10)$$

and

$$Q_3 = g\,\frac{n_3 - 1}{n_3} \qquad (11)$$

If n^x has the values given above,

$$\left.\begin{aligned} Q_1 &= 0{\cdot}999\ g \\ Q_2 &= 0{\cdot}99\ \ g \\ Q_3 &= 0{\cdot}9\ \ g \end{aligned}\right\} \qquad (12)$$

The values of r are found by subtraction between the equations of (12) as is readily indicated in equation (8); and are

$$\left.\begin{aligned} r_1 &= 0{\cdot}009\ g \\ r_2 &= 0{\cdot}09\ \ g \\ r_3 &= 0{\cdot}9\ \ g \end{aligned}\right\} \qquad (13)$$

Since only the galvanometer is shunted, the values of S_1, S_2, and S_3 are easily found.

Armour Institute of Technology,
September 16, 1897.

ART. XVIII.—*Mineralogical Notes on Cyanite, Zircon, and Anorthite from North Carolina;* by J. H. PRATT.

1. *Cyanite.*—The mineral to be described occurs on the farm of Tiel Young, near North Toe River, Yancey Co., North Carolina, a few miles from Spruce Pine, Mitchell Co.

Some exceptionally large crystals of a grass-green color were obtained by the author during the summer of 1896 from Mr. M. Alexander, a jeweler in Asheville. After some difficulty the occurrence was located, during the past summer, and the locality has been thoroughly worked by Geo. L. English & Co. of New York, with the result that many good crystals of the cyanite have been obtained.

The mineral occurs in a decomposed mica schist from which the single crystals are easily separated. The crystals are often intergrown and masses of these were obtained as large as one's two fists. All of the crystals found were of a rich grass-green color and many were quite transparent. A few of the crystals showed a deep blue center with the grass-green margins. The crystals vary in size from $1\times\cdot7^{cm}$ to $3\cdot2\times6^{cm}$; a few coarser ones were observed that were considerably larger. The faces are generally smooth, giving fair reflections of the signal on the reflecting goniometer.

The forms observed on these crystals are as follows:

c, 001	*a*, 100	*M*, $1\bar{1}0$	
b, 010	*m*, 110	*Q*, 120	*t*, 520

The face 520 is apparently a new one for cyanite. The three pinacoids were the only faces observed on the majority of the crystals, some of which were doubly terminated. After a very careful examination of the terminations, they were decided to be real basal planes and not cleavage surfaces. On one of the crystals, all of the faces observed were developed.

The following table shows the identification of the forms by calculated and measured angles. In obtaining the calculated angles the elements given in Dana's Mineralogy (1892), deduced from Rath's measurements* on Greiner crystals, have been used.

$a:b:c=0\cdot89938:1:0\cdot70896$; $\alpha=90^\circ\,5\frac{1}{2}'$; $\beta=101^\circ\,2\frac{1}{4}'$; $\gamma=105^\circ\,44\frac{1}{2}'$

	Calculated.	Measured.
$a\wedge m$, $100\wedge110$	34° 17′	34° 17′; 34° 20′;
$a\wedge M$, $100\wedge1\bar{1}0$	48 18	48 43; 48 41; 48° 45′
$a\wedge Q$, $100\wedge120$	48 43	48 50
$a\wedge t$, $100\wedge520$	17 9	17 13
$a\wedge b$, $100\wedge010$	73 56	73 40; 73 38; 73 42
$b\wedge M$, $010\wedge1\bar{1}0$	57 46	57 41

* Zs. Kr., v, 17, 1880.

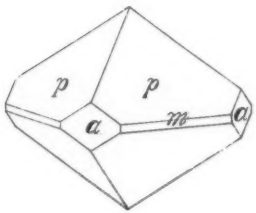
The specific gravity is 3·64 and was determined upon several different samples. Iron and chromium were very carefully tested for, but no trace of either was observed in the analysis, which showed only the presence of alumina and silica.

Pale green cyanite has been obtained from a number of localities and it has also been found in the vicinity of the green cyanite locality on the farm of Isaac English of Spruce Pine, Mitchell Co., imbedded in an undecomposed mica schist. Another occurrence that is worthy of note is that of Graves Mt., Georgia. At this locality thin small plates of the pale green cyanite are associated with well crystallized rutile.

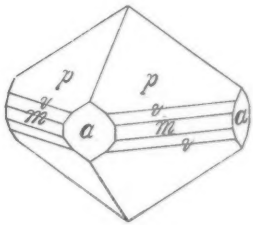
Zircon.—Some specimens of zircon crystals were obtained of Messrs. Geo. L. English & Co., from New Stirling, Iredell Co., North Carolina, and on account of their size and development it has been thought a description of them would be of interest.

The crystals are all very similar in their habit, in which the unit pyramid is strongly developed while the prisms faces are short, figs. 1 and 2. Fig. 1 represents the majority of the

1.



2.



crystals, where the prism of the first order is only slightly developed, at times being hardly perceptible. Those represented by fig. 2 are similar in their habit to some zircon crystals described by the author from the townships of Dungannon and Faraday, Ontario.*

The following forms were observed on these crystals:

a, 100; m, 110; p, 111; v, 221; x, 311.

The face x, 311 was only observed on a very few of the crystals and was but slightly developed. The crystals are all well developed with smooth faces, making them well adapted for measurement on the reflecting goniometer. Although the author had no reflecting goniometer at hand, the faces were readily identified by means of the contact goniometer.

The crystals vary in size from 1^{cm} to $2{\cdot}5^{cm}$ in diameter and are of a reddish-brown color.

* This Journal, vol. xlviii, p. 215, September, 1894.

Anorthite.—The occurrence of this feldspar at Buck Creek, Clay Co., N. C., has been mentioned by J. V. Lewis.* The mineral forms with olivine a mass of forellenstein (troctolite) rock, outcropping over an area of about two acres. The particles of feldspar vary in size from that of a pea to some nodules that were an inch and a half long by three-quarters of an inch broad and are separated from the olivine by a zone of fibrous silicates, composed partly of enstatite.

The feldspar has suffered some kaolinization but the interior of the larger nodules is free from all decomposition. By means of the heavy solution a product was obtained for analysis that varied in specific gravity from 2·6995 to 2·7440.

The results of the analysis made by C. H. Baskerville† are as follows:

		Ratio.		
SiO_2	44·05	·754		2·23
Al_2O_3	30·87	·302		·92
FeO	·84	·011		
CaO	17·30	·308	·328	1·00
MgO	·36	·30		
Na_2O	3·65	·057		
K_2O	·83	·009		
Moisture	·35			
Loss on ignition	1·60			
	99·85			

The ratio of $SiO_2 : Al_2O_3 : CaO$, is near to 2 : 1 : 1, which identifies the feldspar as an anorthite.

Mineralogical Department,
North Carolina Geological Survey, December, 1897.

* Bulletin No. 11, N. C. Geological Survey.
† Of the chemical department of the N. C. Geological Survey.

Art. XIX.—*The Source of the X-rays;* by John Trowbridge and John E. Burbank.

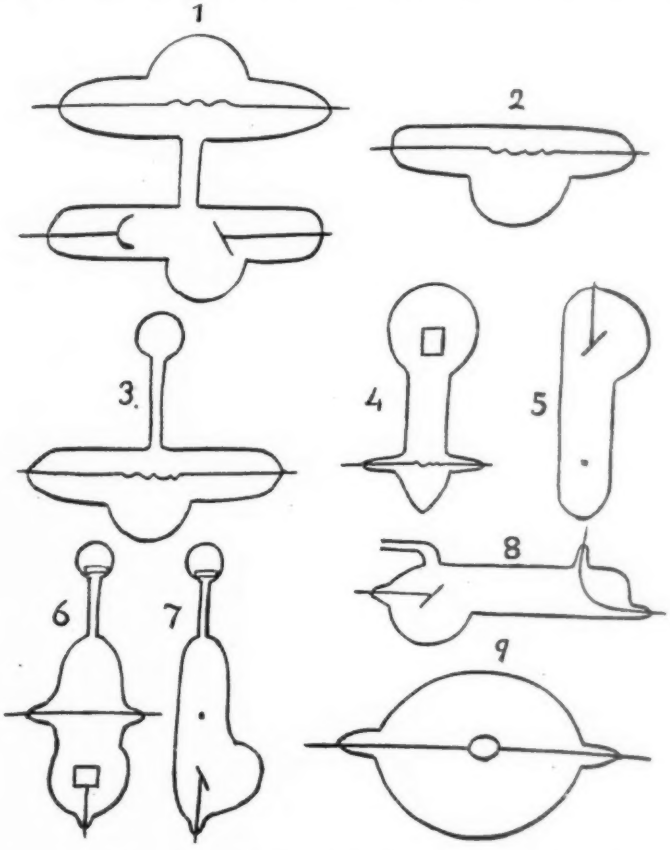
The experiments described in this paper were conducted with Crookes tubes containing no interval between the anode and the cathode: and no discharge therefore in the usual sense occurred in the tubes. A continuous conductor was led through the rarified tube, and it was discovered that the X-rays were given off from every element of this conductor at right angles to its surface when a disruptive discharge occurred in the circuit of which the tube formed a part. This remarkable result was obtained by means of the very high electromotive force obtained by a Planté rheostatic machine which was charged by ten thousand storage cells. We shall describe our experiments with the various tubes we employed and in conclusion state our results.

The first tube is shown in fig. 1 (p. 131). It consisted of a straight wire tube joined to an ordinary Crookes tube of the focus pattern. This latter tube was joined to the straight wire tube in order to test the vacuum in the latter and to be sure that the necessary conditions existed for the production of the X-rays. When the terminals of the straight wire tube were connected to the Planté machine, and the latter was excited, the entire tube fluoresced brilliantly. This fluorescence was especially bright in the connecting tube between the straight wire tube and the Crookes tube, and a beam of light passed across the Crookes tube and formed a fluorescent spot on its bulb.

Photographic plates were exposed opposite to the thin bulb on the straight wire tube. These plates were carefully insulated from the ground and were covered in one case with a sheet of hard rubber $\frac{1}{8}$ of an inch in thickness, and in another case by a sheet of glass about $\frac{1}{2}$ inch in thickness. A powerful brush discharge was seen in the dark room to pass from the thin bulb of the tube to the insulator which covered the plates, and on developing the plates they were found to be covered with star-like clusters surrounded by nebulous patches. It was evident that the brush discharge had produced discharges at the surface of the dry plate, even through plates of glass $\frac{1}{2}$ an inch in thickness. Moreover there was a general darkening of the surface of the plate which indicated the action of the X-rays. No metallic objects could be placed upon the dry plates, for a powerful spark immediately passed to them and punctured the tubes. The darkening, however, was apparently diminished under strips of glass., although the effect of the brush discharge masked the effect of the X-rays.

Before the straight tube was exhausted powerful brush discharges were given off at the ends of the tube. When the tube was exhausted these brushes were much diminished and were replaced by a powerful brush which came off from the straight wire through the bulb of the tube and speedily punctured the latter when any object, even an insulator, was brought within six inches of the bulb. The most interesting results obtained with this form of tube was the production of the so-called X-ray burn by means of the brush discharge from its bulb. When the back of the hand was exposed to this brush discharge, which assumed a peculiar forked nature in the dark-room, a peculiar prickling sensation was experienced and all the symptoms of the well-known X-ray burn developed. The skin when examined under a microscope exhibited an appearance similar to that shown by the photographic plate. There were centers of inflammation surrounded by regions of lesser degrees of burn. It seems evident that the so-called X-ray burn is due to an electrification—a discharge at the surface of the skin—and this electrification may or may not be accompanied by the X-rays. This first form of tube was then abandoned, and a straight wire tube alone (fig. 2) was employed. Similar results were obtained with this tube. It was significant that the whole interior of this tube fluoresced brilliantly when it formed part of a circuit through which a disruptive discharge passed. This latter form of tube was replaced by that represented in fig. 3. A side ending in a thin bulb was added to the straight wire tube. The same phenomenon was exhibited by this tube: in addition a brilliant fluorescence filled the side tube, which appeared to flow in or flow out of the narrow tube which ended in the small bulb. We were reminded by this phenomena of Poynting's hypothesis of the flow of energy into a wire. This form of tube was replaced by a straight wire tube which is represented in fig. 4 and fig. 5 (side view). A straight wire passed completely through the tube and was in circuit with a line on which there was a rapid change of potential. At one end of this tube opposite a thin bulb blown on the tube is a piece of platinum foil inclined like the ordinary focus plane in a focus tube. At first we connected this focus plane with the ground, and having ascertained that X-rays were given off very strongly from this foil, we removed the ground connection and substituted for it a sheet of zinc. The tube still gave off X-rays. We then removed the sheet of zinc and found that X-rays were given off with undiminished strength. This tube was then modified into the form represented in fig. 6 and fig. 7 (side view). The straight wire occupied the middle of a tube: in one end of this tube was placed a focus plane of platinum and at the other end

there was a long, narrow tube which ended in a thin bulb. In this bulb was a crystal of calcite which was confined in the bulb by the narrowness of the bore of the connecting tube. This tube showed that X-rays were given off at right angles to the straight wire, for the calcite fluoresced a brilliant red and the fluoroscope showed X-rays proceeding from the inclined



piece of platinum. This form of tube, moreover, showed that the X-rays are reflected, so to speak, from the interior surface of the glass, for there were multiple shadows of the wire on the sides of the tube which were produced by the X-rays of varying intensity which were developed on the surface of the glass, and which in turn, proceeding from this surface at

different angles, produced elongated shadows. In the next form of tube, the wire passing through the tube was no longer straight, but wae bent in the manner represented in fig. 8. In this form of tube there was a brilliant caustic formed on the sides of the tube opposite the concave side of the wire, and this brilliant caustic threw distorted shadows of the bent wire on the opposite side of the tube. It was evident that the X-rays were given off at right angles to the surface of this wire, and therefore coincided in direction with the lines of electrostatic force. We next experimented with the form of tube represented in fig. 9. This consisted of a large thin bulb five inches in diameter, enclosing a continuous conductor, the center of which consisted of an aluminum mirror. When this tube was exhausted to a very high degree, the mirror formed a bright fluorescent spot on the bulb, the position of which could be readily changed by means of a magnet. When the exhaustion was carried to a very high degree, reversing the current from a Ruhmkorf coil through the tube caused no marked difference in the appearances in the tube: at a lower degree, however, a marked difference resulted. With the employment of a powerful Ruhmkorf coil giving sparks of at least eight inches, X-rays could be detected in this tube, and when the tube was connected to the Planté machine, the X-rays gave strong effects in the fluoroscope.

In order to test the question whether the so-called cathode rays and X-rays are generated primarily only at the cathode, a very large resistance of distilled water was interposed in the circuit with the continuous wire tube (fig. 9) in order to damp any oscillations which might arise. The circuit thus consisted of the tube, the water resistance, a spark gap and the secondary coil of a large Ruhmkorf. The tube was connected at first permanently to the air pump. As the exhaustion proceeded a beam of rays proceeded from the mirror on the continuous conductor which was focused on the wall of the tube. This beam was more brilliant and produced a stronger fluorescence on the tube when the wire was negative than when it was positive. At a higher stage of the vacuum, however, very little if any difference could be detected in the appearance of the tube, and X-rays could be detected outside the tube opposite the fluorescent spot caused by the mirror. That is, the X-rays were given off both when the wire constituted the cathode of the circuit and also the anode. It seems therefore that the term cathode rays is not a general one. It would seem that electric rays might be a more comprehensive one for both cathode rays and X-rays.

Furthermore the phenomenon of electrostatic induction plays an important part in the phenomena of the so-called

X-rays. When the tube represented in fig. 9 had reached a certain stage of exhaustion, a bit of tin-foil connected to a zinc plate 10×16^{cm} and 1^{mm} thick, was stuck upon the outside of the tube where the mirror formed the fluorescent spot. This zinc plate was carefully insulated from the ground. It was seen that a bundle of rays was reflected by the tin-foil to the opposite wall of the tube, showing a well-defined shadow of the mirror and the continuous conductor on this wall. The direction of this shadow could be changed at will by changing the position of the tin-foil. This phenomenon was produced both when the wire was the cathode and when it was the anode. It can be explained on the hypothesis that a layer of electrified particles is held by a condenser action on the wall of the tube and that the fresh coming particles are strongly repelled by those that have accumulated at the spot.

The behavior of aluminum toward the X-rays is so remarkable that it merits especial investigation. Can it be that it manifests a remarkable condenser action toward the high electromotive forces which produce the X-rays, similar to the action which has been observed at lower voltages?* We connected to the air pump, at the same time, two exactly similar tubes, one of which had two pointed terminals of platinum, the other two pointed terminals also; but one consisted of aluminum and the other of platinum. The discharge from a Ruhmkorf coil was sent through these tubes which were in multiple circuit. At a certain stage of the exhaustion it was seen that the discharge passed more easily when the aluminum wire was made a cathode than when it constituted the anode. When the wire terminals in both tubes were made of thin discs, the difference was less marked. This might have been surmised, from previous investigations on the effect of form of electrodes on resulting polarization.† It may be that the anomalous action of aluminum in respect to X-rays is due to a species of dielectric polarization on the surface of the platinum and that thus the surface becomes a new source of electrostatic stress, similar to that which was observed by connecting a bit of tin-foil and a capacity to the tube. Since we are dealing with very high differences of potential and with high charges on the ions, the instantaneous exhibition of electrical energy is very great and might probably explain the diffusion of this energy through the air. According to this hypothesis the light manifestations of the X-rays arise only at the fluorescent screens, or at other suitable surfaces.

* L. Graetz, Wied. Ann., No. 10, 1897, p. 323.
† Karl Robert Klein, Wied. Ann., No. 10, 1897, p. 259.

Conclusions.

1. A Crookes tube enclosing a continuous conductor is well suited, with the employment of high electromotive force, for the study of electric lines of induction.

2. The direction of the so-called X-rays and cathode rays can be changed by electric induction.

3. The so-called X-ray burn can be produced by an intense state of electrification.

4. The so-called cathode rays and X-rays are given off from every element of a continuous conductor at a high stage of the vacuum in a Crookes tube, both when this conductor constitutes the cathode and when it forms the anode of the electrical circuit. The term electric rays, possibly rays of polarization, would appear to be more comprehensive than the terms cathode rays and X-rays.

Jefferson Physical Laboratory, Harvard University.

ART. XX.—*Four New Australian Meteorites;* by HENRY A. WARD, Rochester, N. Y.

[Read before the Rochester Academy of Sciences, Jan. 11th, 1898.]

THREE of the meteorites below described were obtained by me during a trip through Australia in 1896. The fourth was sent to me during the following summer.

Roebourne.

This iron meteorite was discovered in 1894 by Mr. H. Reginald Hester on an alluvial plain 200 miles southeast of Roebourne (the nearest town) and 8 miles from the Hammersley Range of hills, in northwest Australia. The old aboriginals say that it was always there! The specimen was taken by Mr. Hester to Perth, the capitol of West Australia, where it was deposited in the Public Museum and lay for several years. Casts of it had been made in the meantime and distributed to various Australian and European Museums. In June, 1896, I obtained the whole mass from Mr. Bernhard H. Woodward, the curator of Perth Museum, who also gave me the above facts as to the finding.

The mass is shaped a little like the skull of an eagle. Its length is 57^{cm}; its width 34^{cm} and its greatest thickness 7^{cm}. It weighed when entire 191½ lbs. or 86·8 kilos.

Roebourne meteorite, 1 ft. 10½ in. long.

The entire surface is comparatively smooth, with the exception of the lower edge, which is characterized by large and deep pittings, and further has the appearance of a portion having been separated at this point either before or shortly after

its reaching our atmosphere. A most interesting feature of the iron is noticed on the small end (see cut), where there is a prominent ridge or selvedge, having the semblance of being formed by the metal fusing and then being turned over the edge. The length of this ridge, following around the curve, is 27^{cm}, and it extends from 1 to 3^{cm} inwards from the edge. Nothing but a deep surface melting can have caused this. The entire surface is of a lustrous reddish-brown hue, with the metal showing prominently through it in some places.

The Widmanstätten figures are quite regular in size, but very dim on account of the great number of small flakes, probably of schreibersite, scattered through the mass. As far as sectioned, the troilite nodules have been few in number and very small in size, the largest being only 12^{mm} in diameter.

This iron is octahedral.

Analysis of the Roebourne Meteorite by Mariner and Hoskins, Chicago, Ill.

Fe	90·914
Ni	8·330
Co	·590
P	·156
S	trace
Mn	trace (?)
Si	·010
C	trace
	100·000
Specific gravity	7·78

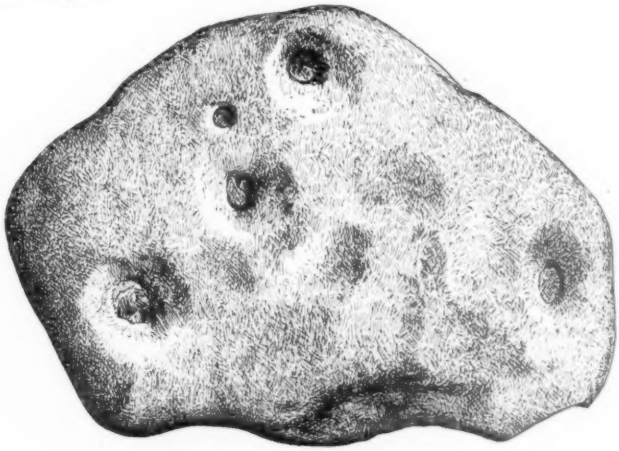
Ballinoo.

This siderite was found early in 1893 by George Demmack, a shepherd, on the water-wash of a tributary of the Murchison River, about ten miles south of Ballinoo, West Australia. It was taken to the Public Museum of Perth, where I obtained it in June, 1896, together with its history as here given.

The length of the mass was 34^{cm}; its width 27^{cm}; and its greatest thickness 11^{cm}. It weighed 93 lbs. or 42·9 kilos. In appearance it suggests a huge flattened potato.

Its pittings are comparatively few; on face showing in cut, there are but five that are prominent. All of these, however, are of special interest from the fact that they show a pitting within a pitting, the inner one having been formed in each case by the troilite having been worn away to a greater depth than the iron surrounding it. That this is the cause is shown clearly on a cut surface parallel to the pitted face (shown in cut) and 5^{cm} from it. Here a troilite 3^{cm} in diameter corresponds with the large pitting in lower left-hand corner of

the cut. The original coating of the mass has entirely disappeared, except in the larger and more prominent pittings and a few small blotches on the smooth parts, where it has a graphitic black, granulated appearance. The oxidized surface is very thin, the metal showing clearly through it on all prominent ridges.



Ballinoo meteorite, ½ natural size.

The Widmanstätten figures, though well developed on etching, are very minute and are scarcely visible to the naked eye. This iron is octahedral. The etched surface also shows numerous sections of troilite not more than 1^{mm} thick and up to $2\frac{1}{2}^{cm}$ long, surrounded by schreibersite. On one section there is a troilite nodule 1^{cm} thick entirely surrounded by a layer of what seems to be graphite, averaging 1^{mm} in thickness. On the same section there are also thin patches of this graphite (?) from 2 to 3^{mm} in diameter.

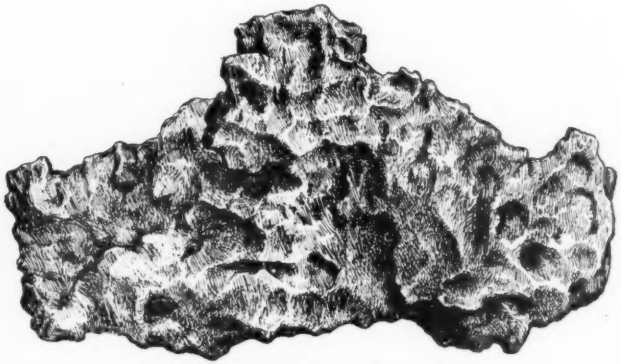
The markings on this iron are more nearly identical with those of Cowra than any other iron with which I am acquainted.

Analysis of the Ballinoo Meteorite by Mariner and Hoskins, Chicago, Ill.

Fe	89·909
Ni	8·850
Co	·740
P	·501
C	trace
Cu	trace
S	trace
Si	trace?
	100·000
Specific gravity	7·8

Mungindi.

A siderite found early in 1897 in Queensland, three miles north of Mungindi P. O., New South Wales. Its length is 39^{cm}; width, 24½^{cm}; greatest thickness, 17^{cm}. Its weight is 62 lbs. or 28·1 kilos. The exterior surface shows in some places the original crust, which is of a dense black, graphitic appearance. But the larger portion of the crust is of a dark yellowish-brown color, while on the prominent ridges it is worn entirely away, thus allowing the edges to show the silver color of the iron within.



Mungindi meteorite, ¼ natural size.

The whole surface on all sides is completely covered with pittings, rather uniform in size, but varying much as to general shape. Some few of the deeper and smaller circular ones, as seen in cut, are undoubtedly formed by the disappearance of the troilite nodules that once filled these cavities. The prominent ridges all have the same general trend, which is in the line of the main axis of the mass.

A cross-section when etched with weak acid brings out, very quickly, Widmanstätten figures of great clearness and beauty: the markings are strongly octahedral. The sectional face as shown in the cut displays several troilite nodules of various sizes and shapes, some of which are long thread-like masses from 3 to 4^{cm} in length, while others are round or oblong in shape. Patches of schreibersite, small in size, are also plainly visible throughout the section, while the Laphamite lines of Smith show distinctly here and there.

As to etching, it is attacked more readily and quickly by weak acid than is any other iron with which I am acquainted.

Another mass of Mungindi weighing 51 lbs. was found at the same time with the one just described. Both masses were discovered by a half-breed and were lying together nearly covered with the red soil.

Mungindi meteorite, section ½ natural size.

Both of these masses are mentioned, without description or analysis, in the Records of the Geological Survey of New South Wales, vol. v, by Mr. George W. Card. Both pieces were subsequently sent to me by Mr. E. F. Pittman, the Director of the Survey, for cutting and distribution.

Analysis of the Mungindi Meteorite by Mariner and Hoskins, Chicago, Ill.

Fe	90·307
Ni	8·230
Co	1·360
P	·093
C	·010
S	trace
Si	trace?
	100·000
Specific gravity	7·4

Mooranoppin.

This octahedral iron was found in or before 1893 by an aboriginal on the sand-plains near Mooranoppin, 160 miles east of York, Western Australia. Its general shape was that of an oblong bar 10^{cm} in length, 8^{cm} wide and 4^{cm} thick. Its weight was $2\frac{1}{2}$ lbs. The original crust is shown on exterior surface of a brownish black color.

The blades of kamasite are so broad that the Widmanstätten figures are not prominently clear on an etched surface. The sections are penetrated by numerous fissures, the largest of which are 1^{mm} wide and 3^{mm} long. These fissures are filled with a black graphitic mineral. A nodule of the same material $1\frac{1}{2}^{cm}$ in diameter occurs at one end of the section and is surrounded by a band of troilite. A few small nodules of troilite, ranging from 1 to 3^{mm} in diameter, occur also sparingly in the section.

This entire mass was obtained by me from the Perth Museum in 1896. I have since cut it into slices.

ART. XXI.—*Measurement of Self-Inductance by Alternating Current and Electrodynamometer;* by K. E. GUTHE.

IN a recent article in this Journal (December, 1897), Professor H. A. Rowland publishes a number of methods for the measurement of inductances and obtains results of far greater accuracy than is possible with the methods commonly used. The principle on which he bases his experiments may here be stated in a more general form:

If alternating currents of the same period but of different phase, traverse the coils of an electrodynamometer, the deflection is proportional to the product of the maximum intensities of the currents, multiplied by the cosine of their difference of phase, or

$$d = a\,\mathrm{II}'\cos\theta$$

It is apparent that we have no deflection, if the difference of phase is 90°, and it is this special case which we employ in practice. The above rule has first been formulated by A. Overbeck,* and was also applied by him to measurements of capacities and inductances which he arranged in various ways, especially in the form of a Wheatstone's bridge. Since the number of resistance boxes to be used in that arrangement decrease the accuracy of the method appreciably on account of their self-inductance and capacity, I was led last summer to try the three simplest arrangements possible, two of which have been given in Rowland's list (3 and 6). Before describing the third method, which I consider the simplest of all, I will give some of the practical details of my work with the first two.

As the source of the alternating current we use a small laboratory-alternator, which makes about 1850 revolutions per minute.

The electrodynamometer is of the Edelmann type and its movable system is replaced by a few turns of fine copper-wire wound on a light wooden frame, having a resistance of 156 ohms. Currents of ·00001 ampere can easily be detected but the self-inductance of the coils is quite appreciable, for the stationary coil ·116 henry and for the movable a little less than ·1 henry. Therefore the correction for self-induction in series with the condenser must be applied. It may be mentioned that the correction as given in Rowland's paper for method 3, in which the self-inductance is put in parallel with the capacity, is wrong; the correct formula is

$$\frac{\mathrm{L}'}{c} = r\mathrm{R}'\left(\frac{1}{1-b^2lc}\right)$$

* Wied. Ann., xvii, p. 816, 1882.

For great sensitiveness we need a rather large condenser. The absorption, as was also mentioned in Rowland's paper, has a very disturbing influence. In a former paper* I pointed out also the variation of the apparent resistance. The Stanley condensers are remarkably free from these faults.

The work with the two methods referred to showed indeed that they are superior to others and especially Rowland's method 6 gave good satisfaction. In this the capacity and the movable coil (r) in parallel with a non-inductive resistance (R'') are placed in series with a parallel arrangement of the self-induction and stationary coil (R) with another non-inductive resistance (R').

$$\frac{L}{c} = (R+R')(R''+r).$$

On account of the large self-induction of the coils of the electrodynamometer, which is always measured in addition to $\varkappa$, difficulties arose, when we attempted to measure coils with small self-inductance. The third method I devised enables us to measure these very accurately. It is a simple application of the above mentioned rule.

I use a two-phase generator, consisting of a stationary Gramme-ring, inside of which a two-pole field-magnet rotates. This generator has been designed by Professor Carhart and is described by Carhart and Patterson.† It gives two currents differing in phase by 90°. The E. M. F. of the generator can easily be adjusted to any desired value up to 60 volts by varying the current through the movable field magnets. One of the currents we send through the stationary, the other one through the movable coil of the electrodynamometer. By inserting resistance in series with the stationary coil, a balance is easily obtained. We connect then the coil, whose inductance we wish to measure, in series with one of the branches and add resistance, until the balance is again obtained. By using a standard induction we can determine the increase in resistance necessary to balance a given increase of self-inductance. For any given frequency, slight variations of which do not appreciably affect the result, the formula is

$$L_1 : L_2 :: R_1 : R_2$$

To show the delicacy of the method I add two sets of observations, taken on different days.

* Electrical Engineer, Sept. 16th, 1897.

† Electrical Measurements, p. 114, or Phys. Rev., iii, p. 141, 1895.

Let R' be the resistance necessary to balance the electrodynamometer,
L the known self-inductance added,
R'' the resistance, to produce the new balance,
R_1 the increase in resistance,
R the resistance corresponding to ·01 henry,
$R''_\times$ and $R_\times$ the values for the resistances obtained with $\asymp$ in series.

R'	L	R''	R_1	R	$R_\times''$	$R_\times$	$\asymp$
180·8	·02	212·1	31·3	15·65	198·45	17·7	·0113
179·97	·035	234·92	54·95	15·7	197·62	17·65	·01121

Experiments made by students using other methods (see Carhart and Patterson's measurements) gave for the same coil values lying between ·0107 and ·0117 henry as the extreme limits, but mostly nearer to the average, which exactly agrees with the second value given above.

The capacity and the self-inductance of the ordinary resistance coils exclude a still greater accuracy, but I feel sure that this can be obtained by employing resistance free from those faults. A preliminary experiment with a fine wire gave as the length that had to be added, when the self-inductance of the standard was increased from ·005 to ·015, from ·015 to ·025 and from ·025 to ·035 henry, 810, 812, 814 divisions respectively.

I believe this method to be of some advantage, when we have an electrodynamometer of large self-inductance. The possible errors are small, since only one resistance-box is used for the adjustment, and the experiment can easily be performed by even inexperienced students.

Physical Laboratory of the University of Michigan.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the Influence of Proximity on Voltaic Action.*—Some time ago Gore showed that when an electrolyte was used in the form of a vertical column about three meters in height, the electrodes being as far as possible perfectly similar, the difference of pressure at its upper and lower ends, due to the action of gravity, produced an electromotive force giving a feeble current on closed circuit. Reasoning that if by producing pressure, gravity exerts an influence on chemical and voltaic action, even an excessively minute one, similar effects should be produced when a large mass, either of metal or other substance, acts by gravity on a voltaic electrode placed at the end of a horizontal column of electrolyte, the author has made a series of experiments to test the question and gives in his paper results which go to establish the correctness of his conclusion. — *Phil. Mag.*, V, xliii, 440–457, June, 1897. G. F. B.

2. *On the Action of Light on Chlorine and Hydrogen.*—It has been shown by Gautier and Hélier that when equal volumes of hydrogen and chlorine, both carefully dried, are submitted to diffused daylight for ten or twelve hours, the quantity of hydrogen chloride formed corresponds to only about 2·55 per cent of the mixture employed; while if the gases be moist, the amount of hydrogen chloride produced under precisely similar conditions may rise even to 60 per cent. Exposed to diffused sunlight for an hour, 6·7 per cent of the mixed gases entered into combination when they were dry and 92·5 per cent when moist. Hence it appears that when the vapor of water is present, the combination is materially facilitated; a result which the authors regard as due to the formation of intermediate products, for instance, hydrochloric acid. In subsequent experiments, the dry mixture of gases was exposed to artificial light. Using an albo-carbon flame at the distance of a meter, the light emitted being equivalent to 14·5 carcels, it was observed that after 237 hours 92·5 per cent of the hydrogen and chlorine had combined to form hydrogen chloride. The rate of this formation increased at first, reaching a maximum, after three hours exposure, of 2·8 per cent per hour. Then it decreased somewhat rapidly. In no case, however, was any limit to the combination indicated; thus showing a marked difference between the action of light and heat. The maximum rate of formation is attained much sooner with sunlight than with artificial light. The authors also observed, that the rate of combination, after the maximum is passed, is not proportional to the uncombined hydrogen and chlorine, since the hydrogen chloride already formed, while it does not limit the action, exerts a decided retarding influence upon the union. Moreover, when the chlorine is in excess, the combination is more rapid. Thus if the

mixture contained 54·8 volumes of chlorine, 42·3 volumes of hydrogen and 2·6 volumes of air, the union was complete after an exposure of 31 hours to the albo-carbon light; while if the gases were mixed in equal volumes 250 or more hours were required under similar conditions. If the hydrogen be in excess the effect is similar though less strongly marked.

BERTHELOT expresses the opinion that it is necessary to take into the account in all experiments made on the combination of mixed gases, the possible reactions which may take place between the gases themselves or the products of their reaction, and the walls of the containing vessel. This action in some cases may be simultaneous and not successive, chemical stresses between the gases and the material of the walls, which together constitute a heterogeneous system, determining the occurrence of any change at all. This local action on the walls will occasion local thermal disturbances which may alter the nature or the rate of the change at the point, and which may even lower the initial temperature at which these changes will take place. Moreover, the reaction may be influenced by the condensation of a gas film on the surface of the walls of the tubes. In fact, the action of surfaces more than the action of masses is here to be taken into the account, these surface actions determining the equilibrium of the system.

GAUTIER, in reply, calls attention to the fact, that Berthelot's criticisms refer rather to his early experiments on the combination of heated gases than to the action of light on gases, now under discussion. The function performed by the walls of the vessel, which in his investigations were kept constant in temperature and were not permitted to become heated by the reaction, is, as he believes, to regulate the speed of the combination and not to limit it. Again, carbon monoxide and oxygen show similar phenomena to those given by hydrogen and oxygen; but in the former case there can be no hydrate formed. The same results are obtained with vessels of glass, silvered glass, porcelain or quartz, and they are independent of the mode of cleaning the surfaces to which the gas is exposed.—*C. R.*, cxxiv, 1267–1273; 1273–1276; 1276–1278, June, 1897. G. F. B.

3. *On the Series Spectra of Oxygen, Sulphur and Selenium.*—In continuing their researches, RUNGE and PASCHEN have now examined the oxygen compound line spectrum and have given in tabular form the wave-lengths of the observed lines. Besides the six triplets which were noticed by Piazzi-Smyth, the authors have found seven other triplets in the spectrum, similar in character, the whole thirteen taken together forming two distinct series. As far as can be determined, the differences between the rates of vibration of the three component lines of each triplet, are the same. All the triplets are distributed regularly over the spectrum, the two series which they form being similar to those which have been already observed in the case of magnesium, calcium, strontium, zinc, cadmium and mercury. Moreover, there are in the oxygen spectrum two other series of lines in addition to the triple

series. It is probable that they represent pairs, since in four of the brightest of them their double character has been observed distinctly. The compound spectrum of sulphur is similar to that of oxygen. Like this, it contains two series of triplets, the first of which consists of six triple lines, the second of four. As before, the differences in the vibration-ratios of the three components are about the same, but these differences are nearly five times greater than is the case in the oxygen triplets. Selenium also gives a compound spectrum in which triplets occur, quite similar to those seen in the case of oxygen and sulphur.—*Ann. Phys. Chem.*, II, lxi, 641–686, July, 1897. G. F. B.

4. *On the Production of Electricity by Chemical Means.*—The cuprous chloride carbon monoxide cell of Borcher's has been experimented with by ANDREAS. He used carbon electrodes which were immersed respectively in solutions of cupric chloride in water and of cuprous chloride in hydrochloric acid, the two being separated by a porous jar. To put the cell in action chlorine was passed into the cupric solution and carbon monoxide into the cuprous. The current at first produced diminished rapidly after a time, when it was found that the cuprous salt had been completely oxidized, the carbon monoxide having taken no part in the action. Of the 1950^{cc} used in one case, only 10^{cc} were oxidized to carbon dioxide. The results were similar with platinum, palladium or nickel electrodes, used at various temperatures. The author then made a gas battery by placing two pieces of platinum gauze together, having a piece of filter paper between them moistened with an electrolytic solution. The resistance was low and it gave promising results with coal gas and air, with oxygen and hydrogen, and with chlorine and hydrogen. With the gases under a pressure of 3 atmospheres, the last combination gave an electromotive force of 1·5 volts and a current of about one ampere. Of course, the cost of the platinum would prevent its commercial use economically. Fairly good results, however, were obtained with carbon, sulphurous oxide being passed on the one side and chlorine on the other, both under pressure, into carbon tubes immersed in sulphuric acid. With one ohm resistance in circuit, this battery gave an electromotive force of 0·5 volts, the resulting products being sulphuric and hydrochloric acids. If the electrodes were made very large probably good results could be obtained with air and sulphurous oxide, the products now being only sulphuric acid.—*Zeitschr. Elektrochem.*, iii, 188–192, November, 1896. G. F. B.

5. *On Graphite, Graphitite and Graphitoid.*—From a careful consideration of the evidence put forward for the existence of graphitite and graphitoid as distinct modifications or varieties of graphite, as well as from his own observations made on natural and artificial material, WEINSCHENK concludes that there is no essential difference between these and graphite and that they must be taken, therefore, as being identical with the latter. The property of not swelling up when moistened with nitric acid and

ignited, on which the name graphitite was based, is explained by the fact that the crystals of "graphitite" are smaller and more compact and hence freer from cleavage cracks into which the acid could penetrate. The so-called graphititic acid has been obtained in crystals having the outlines of the original graphitite and with the same optical characters as graphitic acid. Moreover graphitic acid has also been prepared from graphitoid. There is no difference between the readiness with which graphite and graphitite burn, the rate of burning depending on the size, on the scales and on the purity of the material.—*Zeitschr. Kryst. Min.*, xxviii, 291–304; *J. Chem. Soc.*, lxxii, ii, 447, October, 1897. G. F. B.

6. *Manuel d'Analyse Chimique appliquée à l'Examen des Produits Industriels et Commerciaux;* par ÉMILE FLEURENT, Docteur des Sciences, Professeur remplaçant du Cours de Chimie Industrielle au Conservatoire des Arts et Métiers. 8vo, pp. iv, 582. Paris, 1898 (G. Carré et C. Naud. 12 francs).—As he says in his preface, Dr. Fleurent does not offer this manual as a complete treatise on chemical analysis. His object has been (1) to give the best general methods of qualitative and quantitative mineral analysis and of elementary organic analysis, free from unnecessary detail, (2) to avoid long and tedious processes and to furnish only one, or at most only two methods, giving rapidly the most accurate results, and (3) to furnish the industrial chemist engaged in commercial analysis with the necessary methods for rapidly examining fertilizers, vegetable and animal products, fermented beverages and the like. After twenty pages of preliminary observations on the organization of the laboratory, on reagents and their preparation, on taking samples and on the interpretation of the analytical results, the book is divided into two parts, one treating of mineral, the other of organic matter. The former after giving the principles of qualitative and quantitative analysis, considers the analysis of alloys, mineral colors and fertilizers, and cements. The latter takes up first organic analysis and then treats of soils, fertilizers, vegetable and animal products, including starch, fat, sugar, etc., and finally of fermented drinks. Short chapters are also given on the examination of fibers and the testing of petroleum. The book appears to be carefully written, is well illustrated, and will without doubt be of excellent service to the commercial chemist, for whom it was prepared. G. F. B.

7. *Organic Chemistry for the Laboratory;* by W. A. NOYES, Ph.D., Professor of Chemistry in Rose Polytechnic Institute, Terre Haute, Ind. 12mo, pp. xii, 257. Easton, 1897 (Chemical Publishing Co. $1.50).—The publication of handbooks for the organic laboratory in this country is a gratifying evidence of the rapid development of the science of organic chemistry in instruction. The book before us must be regarded as a most valuable addition to existing manuals and is altogether worthy of Dr. Noyes' high reputation. The style is succinct and clear, the classification of subjects admirable, the methods described are the best possible and the illustrations numerous. G. F. B.

8. *Deductive Physics;* by FREDERICK J. ROGERS, M.S. Large 8vo, pp. vi, 260. Ithaca, 1897 (Andrus & Church).—"The object of this volume," the author tells us in the preface, "is to present in *compact*, though logically complete form, the principal facts, laws, definitions and formulas of the science of physics." Unquestionably the two errors to which text-books are most liable are bulkiness with corresponding diffuseness, on the one hand, and compactness with more or less unintelligibility, on the other. To select the right mean between these undesirable extremes is often a difficult matter. The book before us is certainly compact; but as a text-book it seems to us dry. Supplemented, however, as it is intended to be, with experimental lectures, it will, no doubt, serve a good purpose as a compendium of facts and principles for ready reference. It is provided with numerous problems and is well illustrated with diagrams. G. F. B.

9. *Modes of Motion; or Mechanical Conceptions of Physical Phenomena;* by A. E. DOLBEAR, Professor of Physics, Tufts College. 16mo, pp. iv, 119. Boston, 1897 (Lee & Shepard).—A series of discussions to show the close relationships of physical quantities and their nature as modes of motion.

10. *Electrical Wind.*—It is still an open question whether it is possible to electrify air which is entirely free from dust particles. Late investigations by Lord Kelvin tend to prove that air can be electrified. S. ARRHENIUS describes experiments which support the view that the so-called electrical wind or electrical point, working, is due to an electrification of the surrounding medium by means of which the molecules are separated into ions which then manifest their electrical charges and repel one another. An arrangement with fine points which could be electrified was suspended by a torsion wire in a suitable vessel which could be filled with dry air, hydrogen, oxygen and marsh gas. The reaction of the suspended apparatus was found to be proportional to the gas density, and the experiments therefore support the electrical ion theory rather than the dust theory.—*Wied. Ann.*, lxiii, 1897, pp. 305–313.

The results of Arrhenius appear to be contrary to those obtained by E. Warburg,* who finds by the use of an electrometer that dry air freed from dust cannot be electrified and that the bearers of electrical charges in moist air are drops of water. J. T.

11. *Heat produced by the Röntgen rays.*—E. DORN believes that he has detected an effect of heat produced by the Röntgen rays. The method employed to detect the heating was that described by Toepler for the measurement of very small pressures (Wied. Ann., lvi, p. 609, 1895). Two connecting vessels are in communication with a tube containing a column of light, easily moving fluid-like toluol. One vessel is screened from the effect of the X-rays, while the other, which contains a strip of metal, is exposed to their action. A change in the pressure-level was detected and

* Wied. Ann., lxiii, 1897, pp. 411–418.

was measured in heat equivalents by means of an electrical current which was passed through a suitable conductor in the tube which was exposed to the X-rays. The author attributes the movement of the pressure level to heat, and does not refer to a probable electrification of the air in the tube.— *Wied. Ann.*, lxiii, pp. 160–176. J. T.

12. *Effect of Cathode rays on air.*—P. LENARD shows that the cathode rays in air form regions of mist condensation. A jet of steam at a short distance from an aluminum window of a Crookes tube becomes of a bright whiteness and of a cloudy nature. Cathode rays work in this respect far more powerfully than X-rays. A. Paulsen has formed a cathode-ray theory of the Northern lights and he had anticipated a condensation working of the cathode rays. He has also assumed that the rays of the Northern light form clouds.— *Wied. Ann.*, lxiii, 1897, pp. 253–260. J. T.

13. *Electric arc between aluminum electrodes.*—V. v. LANG, in the course of an investigation of the polarization of electrodes between which the electric arc is formed, finds that a steady current can be obtained by means of an alternating current which feeds an electric arc between an aluminum terminal and a carbon terminal. The direct current amounts to about six per cent of the alternating current and flows from the positive aluminum to the negative carbon. This action is the reverse of that noted when an alternating current flows in an electrolytic cell in which an aluminum electrode and an electrode of another metal are placed.— *Wied. Ann.*, lxiii, 1897, pp. 191–194. J. T.

14. *Stratified discharges of electricity in free air.*—MAX TOEPLER shows that the phenomena of stratification are not confined to discharges in rarified media, but can be produced in atmospheric air at ordinary pressures. The discharges were photographed, thin layers of poorly-conducting substances being interposed between the spark terminals.— *Wied. Ann.*, lxiii, pp. 109–116. J. T.

II. GEOLOGY AND NATURAL HISTORY.

1. *On the Geology of Johnson County, Iowa.*—Professor SAMUEL CALVIN has developed some facts of more than ordinary interest in the chapter on Johnson County in the Annual Report of the Iowa Geological Survey (vol. vii, pp. 33–116, 1897).

The classification of the formations, which is particularly remarkable for the intervals not represented, is as follows, viz:

Group.	System.	Series.	Stage.
Cenozoic	Pleistocene or Quaternary	Recent	Alluvian
		Glacial	Loess
			Iowan till
			Kansan till
Paleozoic	Carboniferous	Pennsylvanian	— Des Moines
		Mississippian	— Kinderhook
	Devonian	Upper Devonian	— State Quarry
		Middle Devonian (Hamilton ?)	Cedar Valley
			Wapsipinicon
	Silurian	Niagara	Anamosa
			Le Claire

In the Devonian system an unconformity is recognized as separating the Devonian from the Silurian formation, and the lowest member of the Devonian in this county is the peculiar "Fayette breccia" described by Norton from the Lime county exposure (Iowa Geol. Surv., vol. iv, p. 157). This is shown, both by its fossils and by the presence of the Otis and Independence beds below it in other regions, to be Devonian and near to the horizon of the Hamilton of New York.

Another unconformity is recognized higher up in the Devonian, separating the Cedar Valley formation from the State Quarry. The characters of this latter formation are so peculiar that special attention may be called to them by a few quotations:

"At the state quarries, or North Bend quarries, in sections 5 and 8 of Penn township, there is a body of limestone of Devonian age, possessing marked characteristics which set it off sharply from the rest of the Devonian in Iowa. The formation has a thickness of about forty feet. At present there is some uncertainty as to its exact taxonomic relations.

"On fresh fracture the state quarry rock is light gray in color. In texture it varies somewhat in different beds, but near the middle of the formation it is composed of coarse, imperfectly comminuted fragments of brachiopod shells cemented together, the spaces being filled with interstitial calcite. Among the recognizable species of shells *Atrypa reticularis* is the most common, but some beds contain very large numbers of *Dielasma* (*Cranæna*) *iowensis*. At some horizons shells of an Orthothetes are common. *Orthis impressa* is not rare and *Rynchonella pugnus* (*Pugnax pugnus*) occurs occasionally. The shells, or fragments of shells, making up the limestone are not embedded in a matrix. They are simply piled on each other and cemented together in a manner illustrated by the formation of the modern coquina along the east coast of Florida. The rocks near the middle of the state quarry beds are a brachiopod coquina having the interstices completely filled with crystalline calcite. * * *

"The anomalous relations of this formation, the limited areas to which it is confined, the abrupt manner in which it appears and disappears, sometimes at the level of one member of the Cedar Valley section and sometimes at the level of another, all lead to the conclusion that it was deposited unconformably on the Cedar Valley limestone after the lapse of a considerable erosion interval. The same view is even more strongly suggested by the fact that in certain respects the fauna of the state quarry beds is unique. The deposit near Solon furnishes *Pugnax pugnus* Martin, *Melocrinus calvini* Wachsmuth, and a very peculiar stromatoporoid, none of which is found in the other Devonian formations. Of other species that have a greater vertical range, as for example *Atrypa reticularis*, there is sufficient variation to distinguish them from individuals of the same species found at other horizons. The Orthothetes, so common in the beds in section 5 of Penn township, is associated with Pugnax, and like it is

limited to the state quarry stage. The great mass of cemented crinoidal debris comprising the beds in Graham township and the upper ten or fifteen feet of the formation at the state quarries, has no parallel in any other stage of the Iowa Devonian. The presence of Dipterus, which elsewhere occurs only in the upper Devonian, tends likewise to separate this from the underlying Cedar Valley formation. In this connection it may be noted that the Rhynchonelloid shell *Pugnax pugnus* is a Carboniferous rather than a Devonian type. It is true that this species is found in the upper Devonian of New York, and at the same horizon in Europe; but it is in the Carboniferous, particularly in Europe, that it attains its fullest development. Faunally, therefore, the relations of the state quarry limestone are with the upper, and not with the middle Devonian, as is the case with the Cedar Valley beds; and so all the phenomena relating to this limestone and its interesting fauna seem to require for their interpretation a number of crustal movements and a period of erosion in the Iowa Devonian heretofore unsuspected."

To the reviewer of this report the phenomena of this quarry are recognized as a confirmation of the theory advanced several years ago* that the Cuboides fauna of the Tully limestone in New York, when studied in comparison with the faunas occurring below it in the New York series of Devonian, must have had an origin from the west and north, by the migration of faunas, rather than in any modification of faunas existing in the immediate region previous to the time of the Tully limestone.

The writer has for several years been expecting to find evidence of a barrier having existed in early Devonian time, extending from the Archæan tongue of Wisconsin toward the ancient island of southeastern Missouri. According to my interpretation of the several Devonian faunas of America, a continuity of the faunas, after the Cuboides zone appeared in the New York area, is noticed in the whole extent of the northern Devonian from New York to Iowa, through the Mackenzie river, across to China and on to the European formations as far as Devonshire in England.

This continuity of faunas does not appear in the Middle Devonian formations, but the brake seems to separate the well known New York faunas from those of Iowa and the Mackenzie valley, and Nevada. The barrier, therefore, was supposed to exist somewhere between Iowa and New York. The State Quarry limestone of Iowa has furnished Prof. Calvin with the very evidence I have been looking for. This evidence points to the occurrence of violent disturbance of the preëxisting quiet sea, the removal in places of channels through the deposits, the sudden local and wholesale destruction of life—with the introduction of a fauna new to that region, but which, like the Cuboides fauna proper, is characteristic of the stages following, and not preceding the Cuboides zone in the North American region. H. S. W.

* The Cuboides zone and its fauna, Bull. Geol. Soc. Am, vol. i, pp. 481–500, 1890.

2. *Iowa Geological Survey: Vol. VII, Annual Report, 1896,* with accompanying papers; pp. 1–555, plates i-xi, figs. 1–81, and six folded maps. SAMUEL CALVIN, State Geologist, 1897.—The volume contains the ordinary administration reports with special papers on the detailed geology of the counties of Johnson, Cerro Gordo, Marshall, Polk, Guthrie and Madison. There is also an elaborate report on the Artesian Wells of Iowa, by W. H. NORTON, representing pp. 113–428 of the volume but bound separately. As the records of the wells have been preserved and carefully correlated, they furnish evidence of the underground structure and variation in thickness of the several beds penetrated not previously published. H. S. W.

3. *La face de la Terre* (*Das Antlitz der Erde*) *par* ED. SUESS, *traduit . . . et annoté sous la direction de* EM. DE MARGERIE, *avec une préface par* MARCEL BERTRAND. Tome I, pp. i–xv, 1–835; 2 colored charts, figs. 1–122. (Armand Calin & Cie.) Paris, 1897.—This translation into French of Suess's classical work opens to many English readers, as well as French, a rich storehouse of structural geology. The French translation has been made by M. Em. de Margerie and eight distinguished specialists. Many notes, especially in the way of bringing the bibliographic references up to date, have been made by the editors, and 76 of the 122 figures are new to this edition.

The book is divided into four parts, two of which appear in the present volume. The first treats of the movements of the outer crust of the globe, including an exhaustive study of the evidences of the traditional deluge in the region of the lower Euphrates; the causes (earthquakes), and the nature of the earth movements resulting are discussed. Dislocations, volcanoes, and a discussion of the relations between the movements affecting our senses and the phenomena of dislocation close this part. The second part treats of mountains and the structure and plan of mountains, more fully those of southern Europe, but, also, clearly those of the chief mountain regions of the globe are described and well illustrated. The two remaining parts on ocean and land surfaces are reserved for a second volume.

American geologists will find here brought together a vast amount of accurate details regarding, particularly, European structural geology, the original sources for which are scattered widely through geological literature and thus out of reach of any but the professional geologist. H. S. W.

4. *Boletin del Instituto Geológico de México.* Nums. 7, 8 *y* 9. *El Minera de Pachuca.* 183 pp. Mexico, 1897.—The latest publication of the Geological Institute of Mexico, under the able directorship of Dr. JOSÉ G. AGUILERA, is a quarto volume of 183 pages, with numerous plates and other illustrations, devoted to a discussion of the mineral deposits of the Sierra de Pachuca. The description of the region, together with an excellent half-tone plate, gives a very clear idea of the general physiographical and geological relations. The various systems of mineral veins

are described in detail and a full account given of the augite-andesites and other igneous rocks. The system of exploitation is then discussed and the various metallurgical processes by which the metals are obtained from the ores. In the preparation of this volume the Director, J. G. Aguilera, has been ably seconded by E. Ordoñez, I. O. González, P. C. Sánchez and others.

5. *Zirkelite:* A question of Priority; by Dr. M. E. WADSWORTH. (Communicated.)—In the Mineralogical Magazine, Vol. xi, pp. 86–88 (read June 18, 1895) is described a mineral containing zirconium, titanium, lime, iron, etc., under the name of *zirkelite*. This paper was prepared by my friend, Dr. E. Hussak, and by Mr. G. T. Prior. Later Mr. Prior (l. c. pp. 180–183, read Nov. 17, 1896) published an analysis of the same mineral.

I wish to protest against the use of the name *zirkelite* for this mineral on the ground of the prior use of it to designate a commonly-occurring rock belonging to the basaltic family. When two subjects are so intimately connected as mineralogy and petrography it does not seem to be for the interest of science that names should be duplicated in them. So true is this that I abandoned the name rosenbuschite, which I had given to a class of rocks in honor of Professor Rosenbusch, because only a few weeks previously it had been employed to designate a new mineral.

The term zirkelite was used by me in 1887, or seven years before it was taken by Messrs. Hussak and Prior. (See Preliminary Description of the Peridotites, Gabbros, Diabases and Andesites of Minnesota. Bulletin No. 2. Geological Survey of Minnesota, 1887, pp. 30–32.) It was used to designate the commonly-occurring altered conditions of basaltic glassy lavas which are often called diabase glass, etc. Zirkelite occurs forming the entire mass of thin dikes, and the exterior parts of many dikes of diabase and melaphyr, as well as the surface of old lava flows like the melaphyrs and diabases of Lake Superior, Newfoundland and elsewhere. Zirkelite holds the same relation to tachylite that diabase and melaphyr do to basalt, *i. e.* an older and altered type. The macroscopic and microscopic characters of this rock were given in the place cited above.

The term zirkelite was again used in the same way in my Report of the Geological Survey of Michigan for 1891–1892; (1893, pp. 90, 97, 138, etc.) It was also published in my classification of rocks given in the Catalogue of the Michigan College of Mines (Michigan Mining School), 1891–1892, p. 104; 1892–1894, Table XI; 1894–1896, Table XI. Further the term zirkelite is defined in accordance with my usage in Lœwinson-Lessing's Petrographisches Lexikon, 1893, p. 252; and accounts of it are given in the Neues Jahrbuch für Mineralogie, 1893, II, p. 292, and in Kemp's Handbook of Rocks, 1896, p. 170.

Michigan College of Mines, Houghton, Mich., Dec. 17, 1897.

6. *Krystallographische Winkeltabellen;* by VICTOR GOLDSCHMIDT, large 8°, 432 pp. Berlin, 1897 (Julius Springer).—In this work is presented for the first time a complete table of

angles for all the crystal forms of all crystallized mineral species. Such a table is eminently useful to the crystallographer to determine without laborious calculation whether the forms observed on a crystal are already known or are new. But the construction of a complete table, based on the accepted method of interfacial measurements between crystal faces, was impracticable, since it required the presentation of an enormous number of angles for each form, and would thus be too bulky to be useful. Monographs on various mineral species contained approximations to such tables, but they were not complete even for those species and were widely scattered through the literature. The invention of the goniometer with two circles* by means of which each face on a crystal may be measured independently of all the others, and its position determined with reference to a fixed pole and meridian by two angular coördinates, first made possible the formation of a complete table of angles. For such a table need now contain for each form only these two angular values, ϕ and ρ (corresponding to geographical longitude and latitude for a point on the earth's surface), which fully characterize the form and which may be directly and rapidly compared with the results of measurements. The tables before us contain these values for each well-established form of each mineral, calculated from the axial elements of the substance and the symbol of the form; there are also given for each form several angles which facilitate the comparison of these angular coördinates with the results of the ordinary interfacial measurement; and lastly a set of linear coördinates which enable the rapid plotting of the form in a gnomonic projection. In an introduction of 29 pages necessary explanations of the values contained in the tables are given, and the schemes employed in each system for the calculation of the values are shown. Some idea of the enormous labor represented by this volume may be given by the statement that it contains upwards of 70,000 values; of this number about half are values recurring more or less often, such as angles of 45° and 60°, but the remainder required each a separate calculation. An appendix contains notes on the various minerals, indicating the particular orientation adopted in each case. The volume forms a logical conclusion to the elegant system of crystal measurement and description which the author has developed, and will be a welcome aid to the increasing number of those who are pursuing their crystallographic studies in the fruitful fields he has discovered.

C. P.

7. *Tabellarische Uebersicht der Mineralien nach ihrer krystallographisch-chemischen Beziehungen geordnet;* von PAUL GROTH. Vierte vollständig neubearbeitete Auflage, 184 pp. Braunschweig, 1898 (Fr. Vieweg und Sohn).—The fourth edition of Groth's Mineralogical Tables has just appeared and will be

* For a brief description of this instrument and the method of using it, as well as for references to the original papers concerning it by Goldschmidt and others, see this Journal, vol. ii, 1896, p. 279.

warmly welcomed by all mineralogists. It would be difficult to find in the literature of the science a publication of like scope, which could be compared with it in the extent to which it has contributed to the advancement of the science. Whether we consider the excellence of the classification presented as a whole, or the arrangement and discussion of the characters of the individual groups, or still further, the suggestions made in regard to the probable chemical constitution of many of the individual species, the work has been from its first appearance of the highest value to all interested in the subject. In its present form, it is brought down to the present time, with the introduction of the new species described since the appearance of the last edition in 1889, also the changes among the old species made necessary by recent investigations, and further the new system of crystallographic nomenclature which the author has developed in his valuable *Physikalische Mineralogie.*

8. *Minéralogie de la France et de ses Colonies.* Description physique et chimique des Minéraux; Étude des Conditions géologiques de leurs Gisements. Tome Deuxième; par A. LACROIX, pp. 353–804. Paris, 1897 (Librairie Polytechnique, Baudry et Cie, Éditeurs).—The author is to be congratulated upon the completion of the second volume of his admirable Mineralogy of France. Earlier parts have already been noticed in this Journal and attention has been called to the very large amount of new material which has been brought together here, in regard to the occurrence of mineral species in France and her colonies. The part now issued includes pages 353 to 804, embracing the native elements, the sulphur compounds, and the chlorides and fluorides. Wherever the reader opens the volume, he is sure to find something of peculiar interest, either in the description of the methods of occurrence or in the characters of the minerals themselves. Many excellent illustrations are introduced, a considerable number of them by the photographic process, which is here used to good advantage.

9. *A Description of Minerals of Commercial Value;* by D. M. BARRINGER. 168 pp. New York, 1897 (John Wiley & Sons).—This novel work is intended as a reference book for those practically interested in minerals, especially miners, prospectors, or students in the field. In addition to some preliminary matter of a general nature, it consists for the most part of a series of tables giving the prominent metals in alphabetical order, with the names of the important ores, and for each the prominent physical and chemical characters, arranged very clearly in tabular form.

10. *A Complete Catalogue of Minerals*, compiled by WARREN M. FOOTE. 187 pp. Philadelphia, 1898.—Collectors will find this well edited catalogue of much convenience in their work. Besides various other lists, the most extended one of which is in accordance with the generally accepted system of classification and gives a brief summary of the characters of each species, there is also one including the minerals classified according to composi-

tion under the prominent metal which they contain. Numerous half-tone plates from photographs give satisfactory reproductions of the actual appearance of the specimens.

11. *Synoptical view of the various divisions of Embryophyta siphonogama;* by A. ENGLER, bearing date October, 1898.—In the 165th installment of *Pflanzenfamilien*, Professor Engler presents a detailed account of his reasons for suggesting the classification which he gave in his Syllabus, in 1892 (and in fact earlier), and introduces his statements by a synoptical view of his system. The strictly technical character of the whole memoir renders it unwise for us to transfer any part of it to our pages, but we must call attention to the importance of the communication. All of our readers who are interested in Systematic Botany should undertake an early and thorough study of the synopsis, explanations, and charts. Those who examine this paper by Professor Engler will be impressed by his fairness towards those with whom he cannot agree, and with his singular fairness in pointing out defects in his own system. In view of Professor van Tieghem's important papers already examined in this Journal, the present installment of the Pflanzenfamilien will be particularly welcome to all students of Botany at this time. G. L. G.

III. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Scientific Collections presented to Yale University.*—Professor O. C. MARSH has recently presented to Yale University, for the benefit of all departments of the University, the extensive and valuable collections belonging to him and now deposited in the Peabody Museum, New Haven. The deed of gift is dated January 1st, 1898. The conditions attached to the gift are only such as appear necessary to insure the permanent care and preservation of the collections themselves. The main collections, six in number, include the following:—

(1) *The Collection of Vertebrate Fossils.* This is the most important and valuable of all, as it is very extensive, contains a large number of type specimens, many of them unique, and is widely known from the descriptions already published, mainly in this Journal. In extinct Mammals, Birds, and Reptiles, of western North America, this series stands preëminent. It represents the main results of nearly thirty years explorations in the Rocky Mountain region.

Among the prominent features of this collection may be mentioned: (1) the series of fossils illustrating the genealogy of the horse, as made out by Professor Marsh; (2) the Birds with teeth, nearly two hundred individuals, described in his monograph "Odontornithes"; (3) the gigantic Dinocerata, several hundred in number, Eocene mammals described in his monograph on this group; (4) the Brontotheridæ, huge Miocene mammals, some two hundred in all; (5) Pterodactyles, over six hundred in number; (6) Mosasaurs, represented by more than fifteen hundred individuals; (7) a large number of Dinosaurian reptiles, some of

gigantic size. Besides these are various other groups of Mammals, Birds, and Reptiles.

(2) *The Collection of Fossil Footprints.* These specimens are mainly from the Connecticut Valley, and thus have a special local interest. They form one of the most extensive and complete collections of the kind in this country.

(3) *The Collection of Invertebrate Fossils.* This includes a large number of interesting specimens from many formations and localities, both in this country and in Europe. Among the series of specimens especially valuable may be mentioned several thousand from the Mazon Creek locality in Illinois; a very extensive collection of Crinoids from Crawfordsville in Indiana; the largest collection of nearly entire Trilobites yet discovered; and one of the rarest series of Silurian Sponges known.

(4) *The Collection of Recent Osteology.* This is believed to be the most complete collection in this country for purposes of study, particularly of Mammals, Birds, and Reptiles. The collection is rich in Anthropoid Apes, the Gorillas being represented by no less than thirteen individuals, and the other genera by rare characteristic specimens.

(5) *The Collection of American Archeology and Ethnology.* This collection is noteworthy for its Central American antiquities, several thousand specimens in number and many of them unique. Among them is the famous de Zeltner collection, containing many gold ornaments. The specimens from Mexico are also of great interest, and the series is a representative one. It includes the well-known Skilton collection.

(6) *The Collection of Minerals.* This is a limited collection, but contains many valuable specimens, among them an interesting series of Nova Scotian Zeolites. These were mainly collected by Professor Marsh, before he graduated at Yale, during numerous visits to Nova Scotia.

Besides the six main collections named, are several others of less value, which include fossil plants, casts of fossils, geological specimens, and recent zoölogical material. These, also, are covered by the present deed of gift.

The conditions on which Professor Marsh has given his collections to Yale University, for the benefit of all departments of the University, are few in number, the more important being the following:—

(1) The scientific collections I now give to Yale University shall be kept in the present Peabody Museum building or in additions thereto equally safe from fire.

(2) During my life, these collections shall remain, as now, under my supervision and control, available for my own investigation and description, or for the work of others designated by me.

(3) At my decease, and forever after, these collections shall be under the charge of the Trustees of the Peabody Museum and their successors, and in the special custody of Curators recommended by them and appointed by the Corporation of Yale University.

(4) The type specimens and others of special importance in these collections shall not be removed from the Museum building. Less valuable specimens, however, especially duplicates, may be so removed by vote of the Trustees of the Museum.

2. *The Smithsonian Institution*, 1846–1896. *The History of its First Half Century.* Edited by GEORGE BROWN GOODE, 856 pp. City of Washington, 1897.—The Secretary of the Smithsonian Institution has chosen wisely in the method he has adopted for commemorating the completion of the first fifty years of its active work. A public meeting, gathering representatives from other scientific institutions, might perhaps have given a greater publicity to the event at the time, but it would have failed to have the permanent character which is gained by the distribution of this most interesting volume. It is well that the public at large should be fully informed in regard to the founding of this trust, the admirable way in which it is administered, and the widely different methods in which it is accomplishing the purpose designed by Mr. Smithson—as stated in his will—"of founding an establishment for the increase and diffusion of knowledge among men." Whether we consider the wise-minded and far-sighted generosity of the donor, the exceptionally successful way in which his plans have been carried out, or the high character and ability of the three Secretaries and others in charge of the work, the institution must be regarded as one entirely unique and without parallel.

The preparation of this volume has been unfortunately twice interrupted by the death of those specially occupied upon it. Dr. James C. Welling, one of the regents, undertook the editorial supervision on the plan drawn up by Dr. G. Brown Goode, but he died in September, 1894. The task was then assumed by Dr. Goode himself in addition to his other arduous duties, but in 1896 his life of remarkable activity was prematurely closed. Fortunately, however, at this time the manuscript was so far advanced that it was possible to carry it through to completion and to publish it without delay on the lines he had laid down.

To Mr. Goode we owe several of the most interesting chapters in the work. First, the description of the founding of the Institution (1835–1846); then that of the Establishment and the Board of Regents, with brief sketches of each of them; and, again, a most interesting and appreciative account of the three Secretaries, Joseph Henry, who was appointed in September, 1846, soon after the Act of Congress establishing the Smithsonian Institution was signed by President Polk, and who served until his death in 1878; then following him, Prof. Spencer F. Baird, who served from 1878 to 1888; and finally the gifted gentleman who now occupies the position, Prof. S. P. Langley. The extent, high character and value of the scientific work accomplished by Henry and Baird in their active lives, as well as that which has been and is still being carried forward by Langley, are too well known and appreciated to require special notice here. Another chapter by Dr. Goode describes the Smithsonian building and grounds.

The account of the generous and high-minded Englishman, James Smithson, who founded this trust, is given by Prof. Langley and appropriately forms the opening chapter of the volume.

It would be interesting here, were it possible, to quote from it, and further to follow out the successive steps in the establishment of the Institution as traced by Dr. Goode, from the death of Smithson in Genoa, in 1829, and the announcement of the Smithson bequest in September, 1835, to the final Act of Congress by which the Institution was established in 1846. But those interested must turn for these facts to the volume itself.

Probably few people not immediately connected with the Institution appreciate how actively its work is carried on, and through what a wide range of departments its influence is felt. The reader who will study Chapters VII to XIV will gain a new insight into the subject, and appreciate in how remarkable a manner Smithson's bequest has borne fruit. We have described here the founding and development of the Library; again the growth of the Scientific Collections from the earliest years, expanded, as they are now, into the United States National Museum; the work in the study of American Ethnology; the remarkable system of International Exchanges for scientific publications; the Astro-Physical Observatory; the National Zoölogical Park, founded within recent years; the work of the Institution in exploration in many fields; and, finally, its various series of publications, by means of which it puts in the hands of the public many papers and volumes which would otherwise be inaccessible or perhaps not brought to light at all.

The latter portion of the volume is devoted to chapters which summarize the work which has been accomplished by the Institution in the various departments of science, each written by a specialist in that line; also other chapters, as one by President D. C. Gilman, showing what the Smithsonian has done in coöperation with other institutions of learning, and another by Dr. J. S. Billings describing its influence in the development of libraries, the organization of societies, and the publication of scientific literature in the country.

A striking proof that the value of the work of the Institution has been thoroughly appreciated by those who have had the opportunity to be near it, is seen by the numerous gifts that have been made to it by private individuals for particular ends, usually in the form of bequests, but also by living donors, as the gift of $5000 by Alexander Graham Bell for astro-physical research. The most important of all the bequests received was that establishing the Hodgkins fund of $200,000, subsequently increased by $50,000 more.

It is obviously impossible within the limits of the present notice to do full justice to this beautiful volume, with the large number of interesting facts which it contains, and the numerous fine portraits of those gentlemen who have been most closely connected with its history. It must suffice if this summary of its contents shall have accomplished the object of leading those interested in science to inform themselves of the great work which the munificence of Smithson has made possible.

3. *Wiedemann's Annalen, Jubelband.* 436 pp. with index, 1897.—Following an earlier precedent, the fiftieth anniversary of the doctorate of Gustav Wiedemann is now commemorated by the publication of a special volume of the Annalen der Physik und Chemie; this forms vol. lxiii of the regular series.

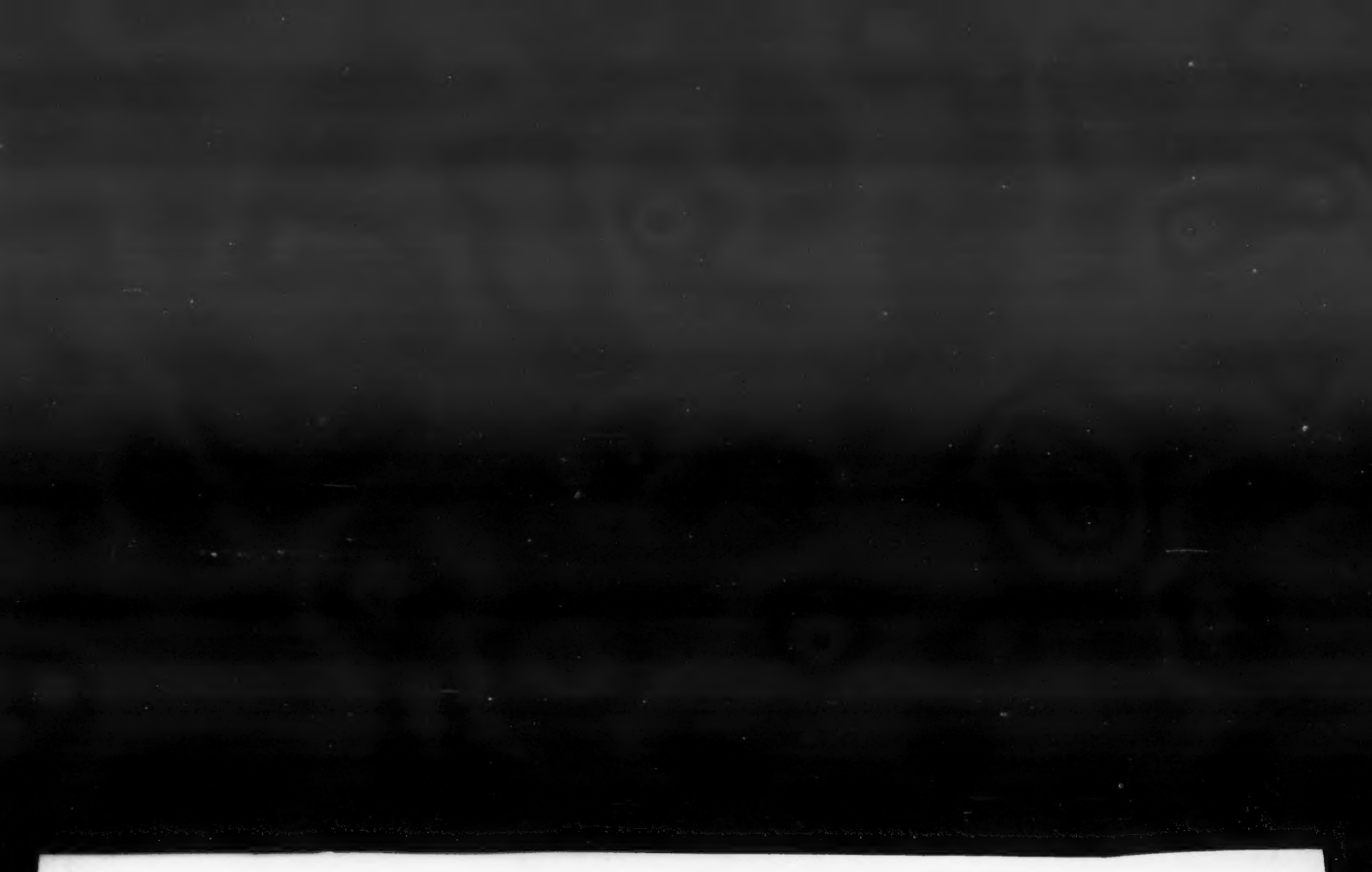
Like all the publications of this honored Journal it is crowded with original matter of importance to physicists; fifty-seven articles are here included. The labors of Dr. Wiedemann, both as author and editor, are well known, but it may be noted that a few years since, on the publication of the fiftieth volume of Wiedemann's Annalen, a summary of his work was given by Hermann von Helmholtz.

4. *Untersuchung über die Bahn des Cometen, 1822, IV;* von Dr. Albert Stichenoth, pp. 64, quarto. Leipzig, 1897 (W. Engelmann).—This comet, which was discovered by Pons and observed for nearly four months at sixteen observatories, was investigated by Bessel with Besselian thoroughness. The present treatise, which is among the most laboriously complete to be found in comet literature, and admirably planned, introduces the small effect of perturbations and is of interest as settling the question of the eccentricity. Bessel's eccentricity of 0·9963021 (period 5449 years) is wisely made the independent variable of the normal differential equations and the effect on the normal equations of 9 values of ϵ from 1·0 to 0·9943 determined.

It is thus shown that the normal equations are satisfied within reasonable limits of error by any period from 4504 to 8748 years, but not by a parabola. W. B.

5. *Problems of Nature, researches and discoveries of Gustav Jaeger, M.D., selected from his published writings;* ed. and transl. by Henry G. Schlichter, pp. 1–261. (Williams & Norgate, London), 1897.—The book contains thirty-two chapters on various topics, illustrating the views of a thoughtful, educated physician on matters suggested by the investigations of Darwin and others during the last fifty years.

6. *Verzeichniss sämmtlicher Preparate, Drogen und Mineralien mit Erläuterungen,* 1897.—A new edition of the well-known Merck catalogue of chemical preparations, drugs and minerals.

OBITUARY.

Dr. Albrecht Schrauf, Professor of Mineralogy at the University of Vienna and for many years Custos in the Imperial Museum, died in December last at the age of sixty years. He was an active worker and profound thinker, the author of many mineralogical memoirs and of several books, the most important, his Lehrbuch der physikalischen Mineralogie (1868) and his Atlas der Krystallformen (1865–1877); the last was never completed.